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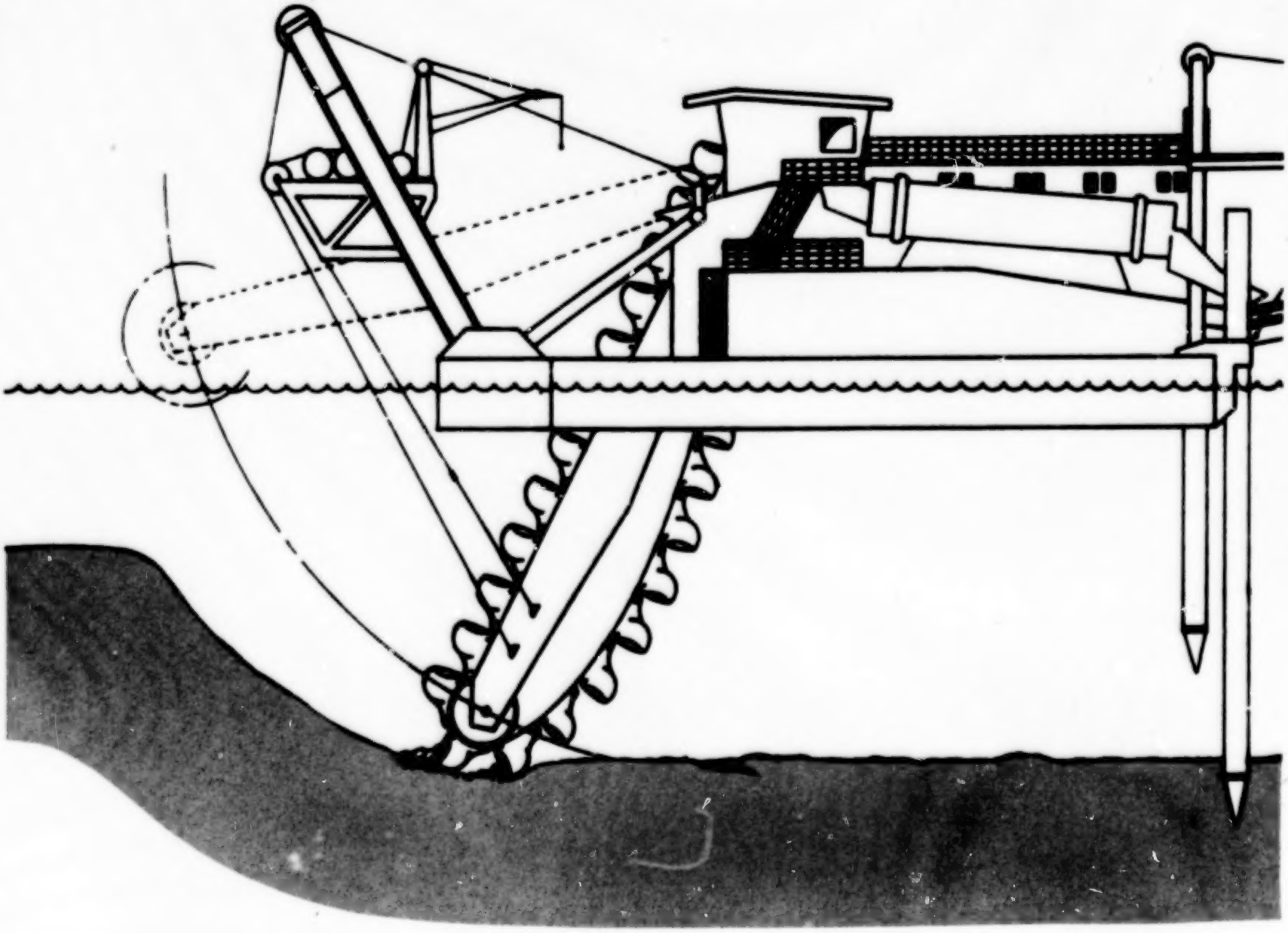
WORKSHOP TO DESIGN BASELINE AND MONITORING STUDIES FOR THE OCS MINING PROGRAM NORTON SOUND, ALASKA

Workshop Proceedings



July 1990

MMS U.S. Department of the Interior
Minerals Management Service
Alaska OCS Region

COMPLETED

OCS Study MMS 90-0059

WORKSHOP TO DESIGN BASELINE AND MONITORING STUDIES FOR THE OCS MINING PROGRAM NORTON SOUND, ALASKA

WORKSHOP PROCEEDINGS

November 30 to December 1, 1989
Anchorage Hilton Hotel
Anchorage, Alaska

Prepared for:

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Alaska OCS Region
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July 1990

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WORKSHOP TO DESIGN BASELINE AND MONITORING STUDIES FOR THE OCS MINING PROGRAM, NORTON SOUND LEASE SALE

November 28 - 30, 1989
Anchorage Hilton Hotel
Anchorage, Alaska

TUESDAY NOVEMBER 28, 1989 - DENALI ROOM

8:30 - 8:45 AM	**REGISTRATION**
8:45 - 10:00 AM	**INTRODUCTION**
	Introductory Remarks - Cleve Cowles (MMS)
	Opening Remarks - Workshop Chairman, Don Hood (Private Consultant)
	Workshop structure, products and logistics - Chuck Mitchell (MBC)
	Previous and Current Sampling Results - Eric Crecelius (Battelle)
10:00 - 10:30 AM	**SESSION 1 - WATER QUALITY AND SEDIMENTS**
	Session Chair - Eric Crecelius, Ph.D. (Battelle)
	Assistant Chair - John Trefry, Ph.D. (Florida Institute of Technology)
10:30 - 10:45 AM	**BREAK**
10:45 - 12:00 PM	**SESSION 1 CONTINUATION**
12:00 - 1:15 PM	**LUNCH**
1:15 - 2:15 PM	**SESSION 1 CONTINUATION**
2:15 - 3:15 PM	**SESSION 2 - FOOD CHAIN/BIOACCUMULATION**
	Session Chair - Ron Eisler, Ph.D. (USFWS)
	Assistant Chair - Anton Scheuhammer, Ph.D. (Canadian Wildlife Service)
3:15 - 3:30 PM	**BREAK**
3:30 - 5:00 PM	**SESSION 2 CONTINUATION**
5:00 PM	**ADJOURN**

WORKSHOP TO DESIGN BASELINE AND MONITORING STUDIES FOR THE OCS MINING PROGRAM, NORTON SOUND LEASE SALE

November 28 - 30, 1989
Anchorage Hilton Hotel
Anchorage, Alaska

WEDNESDAY NOVEMBER 29, 1989 - DENALI ROOM

8:30 - 10:15 AM	**SESSION 3 - HUMAN HEALTH** Session Chair - David Marsh, M.D. (University of Rochester, School of Medicine) Assistant Chair - Tom Kosatsky, M.D. (Montreal General Hospital, Community Health Department)
10:15 - 10:30 AM	**BREAK**
10:30 - 11:30 AM	**SESSION 3 CONTINUATION**
11:30 - 12:15 PM	**SESSION 4 - HABITAT ALTERATION** Session Chair - Sathy Naidu, Ph.D. (University of Alaska, Fairbanks) Assistant Chair - To be announced
12:15 - 1:30 PM	**LUNCH**
1:30 - 3:30 PM	**SESSION 4 CONTINUATION**
3:30 - 3:45 PM	**BREAK**
3:45 - 5:00 PM	**SESSION 5 - STATISTICAL AND SAMPLING TECHNIQUES** Session Chair - Woollcott Smith, Ph.D. (Temple University) Assistant Chair - Robert Smith, Ph.D. (EcoAnalysis, Inc.)
5:00 PM	**ADJOURN**

WORKSHOP TO DESIGN BASELINE AND MONITORING STUDIES FOR THE OCS MINING PROGRAM, NORTON SOUND LEASE SALE

November 28 - 30, 1989
Anchorage Hilton Hotel
Anchorage, Alaska

THURSDAY NOVEMBER 30, 1989 - DILLINGHAM/KATMAI ROOM

8:30 - 8:45 AM	**WORKSHOP GOALS REITERATED**
8:45 - 12:00 PM	**SESSIONS 1 THROUGH 4**
	Individual group discussions
	Session summary preparation
12:00 - 1:15 PM	**LUNCH**
1:15 - 1:45 PM	**SESSION 5 REVIEW AND RECOMMENDATIONS**
1:45 - 2:15 PM	**SESSION 1 REVIEW AND RECOMMENDATIONS**
2:15 - 2:45 PM	**SESSION 2 REVIEW AND RECOMMENDATIONS**
2:45 - 3:00 PM	**BREAK**
3:15 - 3:45 PM	**SESSION 3 REVIEW AND RECOMMENDATIONS**
3:45 - 4:15 PM	**SESSION 4 REVIEW AND RECOMMENDATIONS**
4:15 PM	**QUESTIONS/DISCUSSION**
5:00 PM	**ADJOURN**

CHAPTER 1

INTRODUCTION

INTRODUCTORY REMARKS

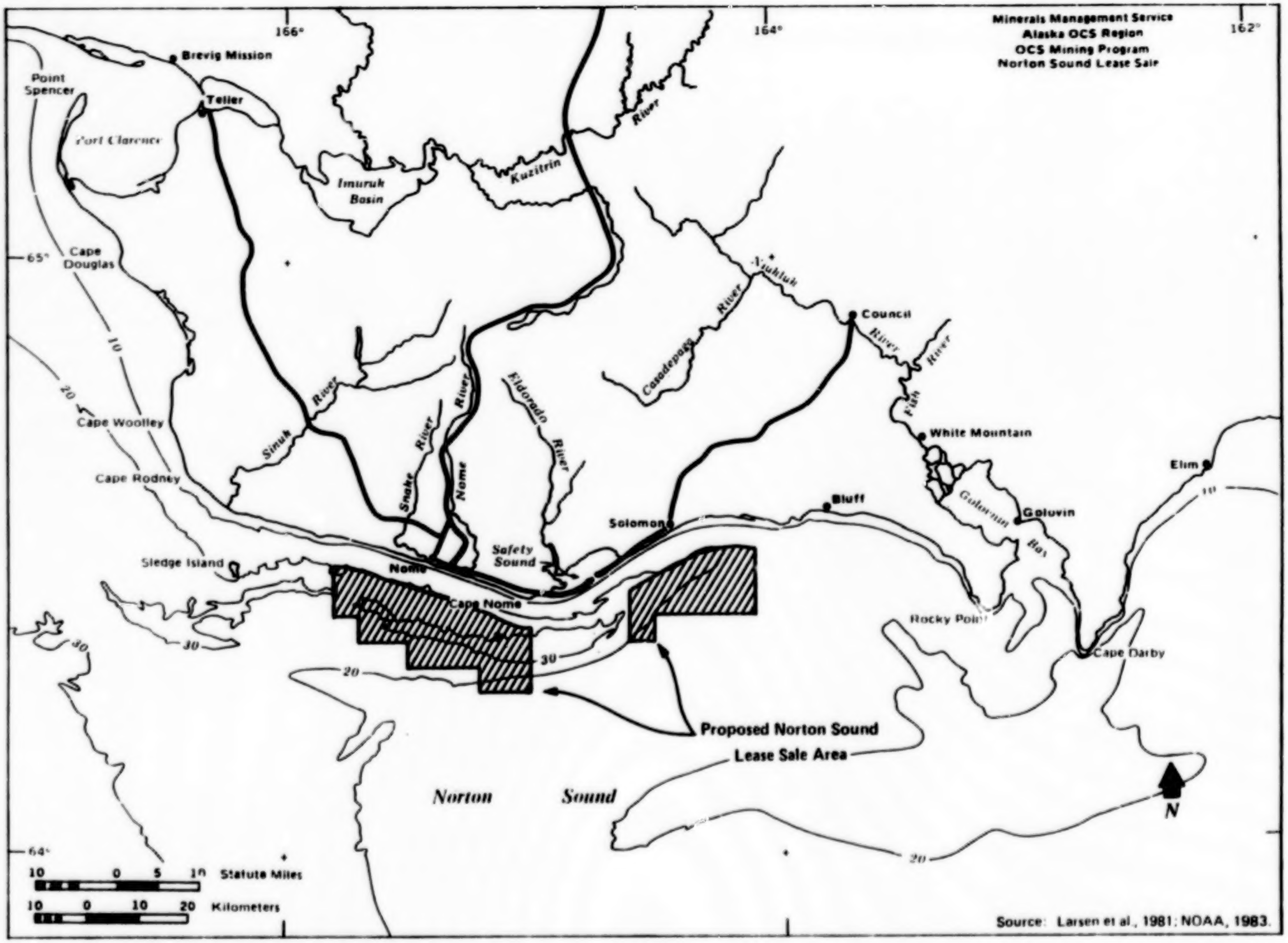
Cleve Cowles, Ph.D.
Chief, Environmental Studies Unit
Office of Leasing and Environment
Alaska OCS Region
Minerals Management Service
Anchorage, Alaska 99508

On behalf of the Alaska OCS Region, Minerals Managment Service, I would like to welcome all of you to this workshop entitled *A Workshop to Design Baseline and Monitoring Studies for the OCS Mining Program, Norton Sound Lease Sale*. In my introductory remarks this morning I would like to give you a brief overview of some of the relevant history that leads us to this workshop, the purpose of our meeting, recognize our noteworthy participants and introduce our workshop chairman.

Because there is sufficient national and industry interest, the Department of the Interior and Minerals Management Service has instituted an outer continental shelf (OCS) mining program to selectively, on a case by case basis, consider commercial scale mining on the OCS. Under consideration, in a related proposed action, is to offer for lease submerged lands on federal lands in Norton Sound near Nome, Alaska (Figure 1-1). This would be for the purpose of mining and recovery of gold and minerals other than oil, gas, and sulfur. In 1987 both industry and the State of Alaska made a request for further federal consideration of the need for mining development and cooperative evaluation of related issues. In addition to the formation of a joint federal-state technical coordination team to share in scoping and evaluative activities, the federal mining leasing process was initiated. The federal government had previously considered and later cancelled four offshore sand and gravel lease sales. The action proposed here is the first for gold or other minerals proposed to date. However the state of Alaska has issued eight offshore mining leases in the Nome and Bluff areas, dating as early as 1966. Offshore mining activities are currently being conducted in one state lease area near Nome.

As part of the federal pre-lease process, a Draft Environmental Impact Statement (EIS) was issued in November 1988, a document with which most of you are familiar. Due to recently acquired information, MMS has decided to re-issue a draft EIS which will take this new information into consideration. As many of you will recall, in late November 1988, about the same time the EIS came out, we held a workshop entitled *Mercury in the Marine Environment*. That workshop was instrumental in leading to further scrutiny of available information and the conduct of additional studies. We recently funded the collection and analysis of water samples collected in June and September 1989, from state waters. Also we supported the collection of human hair samples from local residents in the Nome area to be analyzed for levels of mercury and arsenic. We will have an update on the results of the analytical portion of these studies, which have been completed to date, by Dr. Eric Crecelius of Battelle Northwest Laboratories later this morning. This brief review summarizes the history of previous meetings and the action under the OCS mining program that has occurred up to this point in time.

The purpose of this workshop, as stated in our letter of invitation to our various distinguished guests and session leaders, is to assist the MMS in designing baseline studies in support of the OCS mining program. Presumably the design of baseline studies would be directly related to the design and implementation of any potential monitoring that might be necessary later. Our justification is based on regulatory mandates which detail that monitoring post sale mining activities related to the OCS may be required. These regulatory aspects can be found under 30 CFR Part 282.28 published in the Federal Register Notice dated January 18, 1989.



Source: Larsen et al., 1981; NOAA, 1983.

Figure 1-1. Norton Sound lease sale area.

An important point I want to stress in consideration of our primary purpose is that, although we have structured this workshop to be as broad as possible to encourage multidisciplinary expert input to help us identify the most important components of a future baseline measurement and later monitoring system, the MMS has not committed itself to nor its potential lessees to any potential future monitoring. Future monitoring or baseline measurements will be decided as part of other agency decision processes in terms of the need or scope, and we intend to use the results of this workshop to provide a partial basis for those sorts of decisions. It is conceivable that MMS would identify further study needs, funding, and begin such studies as early as 1991. But again we do not have any specific decision or intent at this point to do so.

The second major purpose of the workshop is to provide an additional opportunity to update our current information and current knowledge pursuant to ongoing environmental assessments. The presentations of our invited guests will serve this purpose. Now I wish to recognize our participants and to give you a feel for the breadth of participation. As you are aware from our agenda, we have a number of invited experts in fields of water quality, sediment chemistry, bioaccumulation of metals, human health, habitat alteration, and statistical design. I would like to thank these individuals for being available and willing to share their expertise with us. Also here today are MMS staff and managers from the Alaska OCS Region Office of Leasing and Environment, and our Office of Field Operations. The Offshore Strategic and International Minerals Office of MMS is also represented. This latter group has provided the funding support for this workshop.

On Wednesday there will be a meeting of the members of the joint federal-state technical coordination team. We hope that many of the members of the coordination team will attend sessions of this meeting and contribute to the subsequent discussion groups. We welcome those of you from the coordination team who are here today. MBC Applied Environmental Sciences is the firm providing meeting coordination, logistics and report preparation support. Also there are numerous other attendees who have chosen to select this meeting. Today there are four other options right here in this hotel, I believe, and we are certainly pleased that your interest in this particular meeting has prevailed. I am sure that you won't be disappointed that you chose to "fly with MMS" for today and the next couple of days. In fact you could probably say that we are offering you a sort of "Gold Coast Service" this morning.

Last but not least, I am very pleased to have the opportunity to introduce our workshop chairman, Dr. Donald W. Hood. For many of us that who have been involved in environmental information issues pertaining to the Alaska OCS over the recent years, Don needs no introduction. Suffice to say his contribution to the science of physical-chemical oceanography of the Alaska OCS has been major and has been recognized nationally and internationally. Dr. Hood's specialty is chemical oceanography which is most germane to the conduct of this workshop. A past director of the Institute of Marine Sciences, Don has also served for many years, in fact, since it's inception on the Scientific Advisory Committee to the BLM and MMS. He has stepped down from that only within the last year. We also know Don well for his work as co-editor for the benchmark volumes, *The Eastern Bering Sea Shelf: Oceanography and Biological Resources* and *The Gulf of Alaska: Physical Environmental and Biological Resources* books. So we are very fortunate and pleased to have Dr. Hood officiate at this meeting. So at this time, Don, if you are willing to lament the position that we have thrown you into this morning, give us a little bit of the background of the objectives of the workshop and continue with the introductions.

4

BACKGROUND

Donald W. Hood, Ph.D.
Private Consultant
Professor Emeritus
University of Alaska

Thanks Cleve. Ladies and Gentlemen. To be Chairman of a workshop pertaining to the environmental quality of Norton Sound in the Bering Sea is a privilege, an honor, and a challenge for one who has spent much of his scientific life studying the waters around Alaska, especially the Bering Sea. I hope I have your patience and sympathy as I undertake this task.

Now I would like to explain to you the objectives of this meeting, but before I do I would like to make a few remarks.

It is not very difficult to lead a workshop when the members of the group of participants are composed of individuals that are world experts on the topics to be considered. We have many such people at this workshop. I had the opportunity to read last year's workshop report on "*Mercury in the Environment*" in preparation for this meeting. It is an outstanding document that contains much information on what we know about mercury in the environment and concerns regarding it. Many of the same people are at this meeting and I'm sure we will continue to deliberate at the same high level.

As you all know, the subject of this meeting in monitoring. We, who are students of the ocean have a great challenge. It lies in the fact, that if man is going to use the ocean, which he certainly does and will, then he has to determine how to examine it for the consequences of his uses. Dredging for gold in the surface sediments off Nome might, at the outset, impress us as being a rather innocuous operation, but herein lies the challenge. If we are going to disturb the ocean, then we are going to have some effect. Our ability, so far, in most monitoring of the ocean endeavors has been limited by our ability to detect change ascribable to the ocean use. In many situations in which I have been involved, after making careful plans, and applying the most advanced techniques, the studies have ended up without being able to show any significant effect. A case at point is the study of the effect of ballast water discharge into Port Valdez (Shaw and Hameedi 1988). The results of these extensive studies are satisfying in that little environmental damage could be detected as a result of this operation, but owing to the dramatic natural changes (recovery of the Port from the 1964 earthquake, re-establishment of sea otters, and increase in salmon from enhancement efforts) in the system over the 15 years of the study any effects there may have been were obscured by natural variability. Often as in the Port Valdez case we end up stating that there is little or no effect and perhaps that is true, but there must be some accumulation of effects through time of use of the ocean. It is our scientific responsibility to do what is needed to assess the effect of an ocean use, and having determined what the effect has been, to state this effect in language that permits intelligent management decisions. We must determine whether an effect is important or not. To escape by saying we cannot see an effect, merely says we aren't good enough to see it. In many cases we lack the technology necessary to accomplish this goal.

I hope this workshop represents the above challenge to all of us. If it does, and we are in a small part successful in meeting that challenge, it will add considerably to our ability to look at the marine environment in a way that is meaningful to those that use it in the future.

The demand for ocean disposal of wastes of all kinds, particularly solids and particulates, will certainly increase in the future. I see no escape from this conclusion. If we consider population trends or technology demands there appears to be no way out. The ratio of one unit of land area to three units of ocean surface make this underutilized resource very attractive. The fact that there is approximately 0.5 km^3 for each human being on earth also provides some grist for ponder. If we accept the fact that the ocean is not sacrosanct, and that for many purposes it can be used to advantage over land resources, many opportunities become evident. The oceans have processed natural wastes for eons and human wastes for centuries. Intelligent use of this resource will allow continuation of waste disposal into the future. Caution is necessary, we must not poison this resource.

Presently we are in a dilemma about ocean use. There are those that feel any discharge to the ocean constitutes pollution and is to be avoided. Much of this attitude comes from a lack of quantitative information regarding the magnitude of the oceans and the processes involved in how it functions. This makes it much more difficult to use the ocean as a resource. Unfortunately, decisions on these matters often fall in the realm of politics, i.e., sewage sludge might be dumped in large quantities off New York City because of the clout represented by economics, but not tolerated in the Bering Sea. I think it is our business to rectify this situation by quantifying the ocean's capacity to accommodate man's uses, and to educate the public at all levels of how ocean processes respond to these uses.

This meeting can add valuable information. To quote from a paper I found in a National Research Council publication written by the Marine Affairs Board in 1989,

> "If the ocean is to receive waste via disposal at sea, the cost effective and scientifically defensible program of environmental monitoring of an ecosystem or regional basis must be developed and put in place. It must be timely and responsive to management needs."

I have added,

> "The public must be kept abreast of program accomplishments. It is important that communications with the public be complete."

Now to get down to the issues of this workshop. Our purpose here is to examine the possible effects of the gold dredging operation on federal lands off the coast near Nome, Alaska and to devise monitoring schemes that will detect and evaluate these effects. Specifically, we must select indicators of possible effects, identify sites, procedures, and methodologies that are appropriate for monitoring purposes under conditions of the proposed operation. We will need to organize our thoughts in such a way that we can, as a product of this meeting, prepare a document containing the deliberations and recommendations of this workshop. It is my function to work toward this end.

Fortunately, there has been previous oceanographic work done in the Nome area in anticipation of the gold dredging operations. The first interdisciplinary study was done by the Institute of Marine Science in 1973 for the American Smelting and Refining Company of the Salt Lake City, Utah (Hood *et al.* 1974). This report contains results and analyses of three cruises taken in the northeast Bering Sea, with emphasis on Norton Sound, during the summer of 1973. The station locations for the physical oceanographic aspects are shown in Figure 1-2. The

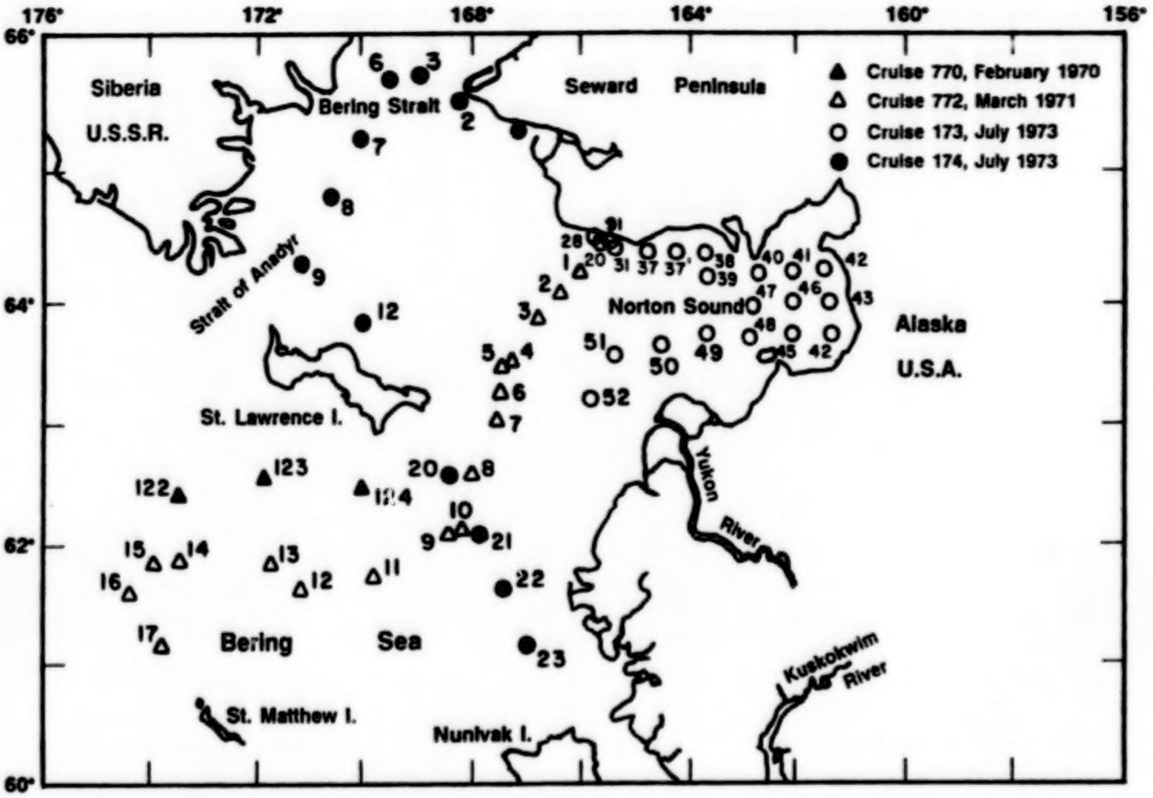


Figure 1-2. Physical oceanography sampling stations.

cruise of the R/V *Acona* in July of 1973 concentrated most of its efforts in Norton Sound, especially in the proposed lease area near Nome. Analyses of the physical data revealed the presence of four types of water structure in Norton Sound, as shown in Figure 1-3. To the east, represented by Stations 40, 41, and 46, is a water mass with a surface layer of salinity of less than 0.5 seawater down to 5 m, a transitional layer down to 10 m, with Bering Sea water at the bottom. This water type is dominated by local fresh water runoff. A second type is represented by Stations 48, 49, and 50. It is of intermediate salinity from top to bottom and is dominated by discharge of the Yukon River. A third type, represented by Stations 1, 31, and 37, is a mixture of Bering Sea and Yukon River water that best typifies the region in the lease area near Nome. This water tends to be isohaline top to bottom (well mixed) owing to strong tidal and wind mixing. The fourth type, represented by Stations 51 and 52, indicates Bering Sea water modified, at the surface, by fresh water inputs. The data collected were insufficient to determine flushing rates or circulation. However, the data do suggest a circulatory movement of Bering Sea water along the bottom and all data indicate a northward transport at the north end of Norton Sound.

The sampling station locations for chemical, biological and geological oceanography for the area immediately off the coast of Nome are shown in Figure 1-4. Chemical data included values for the trace metals Pb, Cd, Cu, and Zn at six stations (2, 3, 12, and 28) at three depths (0, 10, and 15 m), dissolved and particulate organic carbon at six stations (1a, 1b, 3, 13, 26,

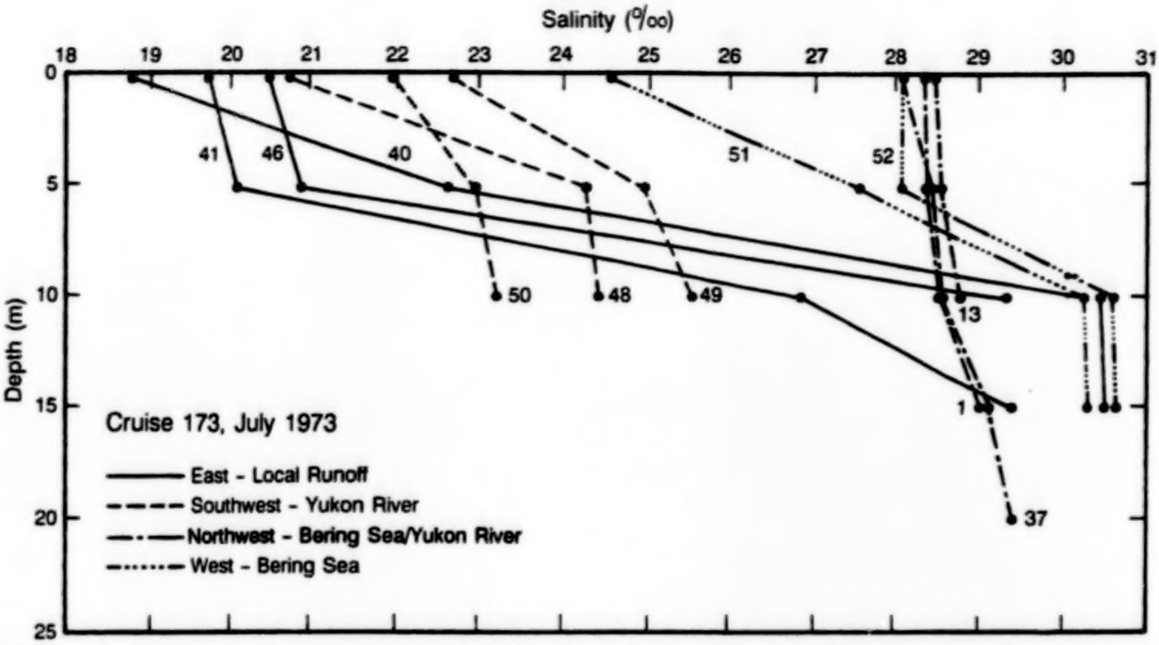


Figure 1-3. Salinity profiles for selected stations from Cruise 173, July 1973, showing four types of water structure in Norton Sound.

and 28), and pH at all stations at multiple depths. Values for the nutrients, NO$_3$-N, Si (OH)$_4$-Si, NH$_4$-N, and PO$_4$-P and chlorophyll at stations 1, 3, 8, 10, 13, 18, 20, and 28 at multiple depths are also given. As is usual for the Bering Sea surface waters for June NO$_3$-concentrations are depressed to near zero and NH$_4$-N is about 10 fold higher owing to recycling in the water column. Chlorophyll ranged between 9.5 and 13.3 mg/m^2.

Biological collections and analyses were made as indicated in Figure 1-3 and the results compared to earlier data reported by Ellison *et al.* (1950) and Geiger and Baxter (1986, unpubl. data). Few species changes were observed except that the Gieger and Baxter data shows that small snow or tanner crabs (*Chionocetes opilio*) frequent the area and two additional species of brachyuran crabs may also occur. The biological data presented in this report are especially valuable for this workshop because not only are species lists for each station given, but also population density and their wet biomass.

Extensive sedimentary data were obtained on cruises to Norton Sound during the summer of 1973 in order to delineate the sedimentary environment between Nome River and Cripple River. The study included bottom topography, grain size distribution, suspended load distribution and heavy mineral concentrations in order to determine the sediment source and the dynamics of sediment transport in the region. The distribution of surficial sediments as determined by these studies is shown in Figure 1-5.

The Hood *et al.* 1974 report also contains a detailed study of the socioeconomic conditions in the Nome community in 1973. This report has sections on Population and Education; Employment and Wages; and, Housing, Public Service, and Transportation, all of which have great value in determining the effects that mining operations on the proposed federal leases may have on the people of Nome.

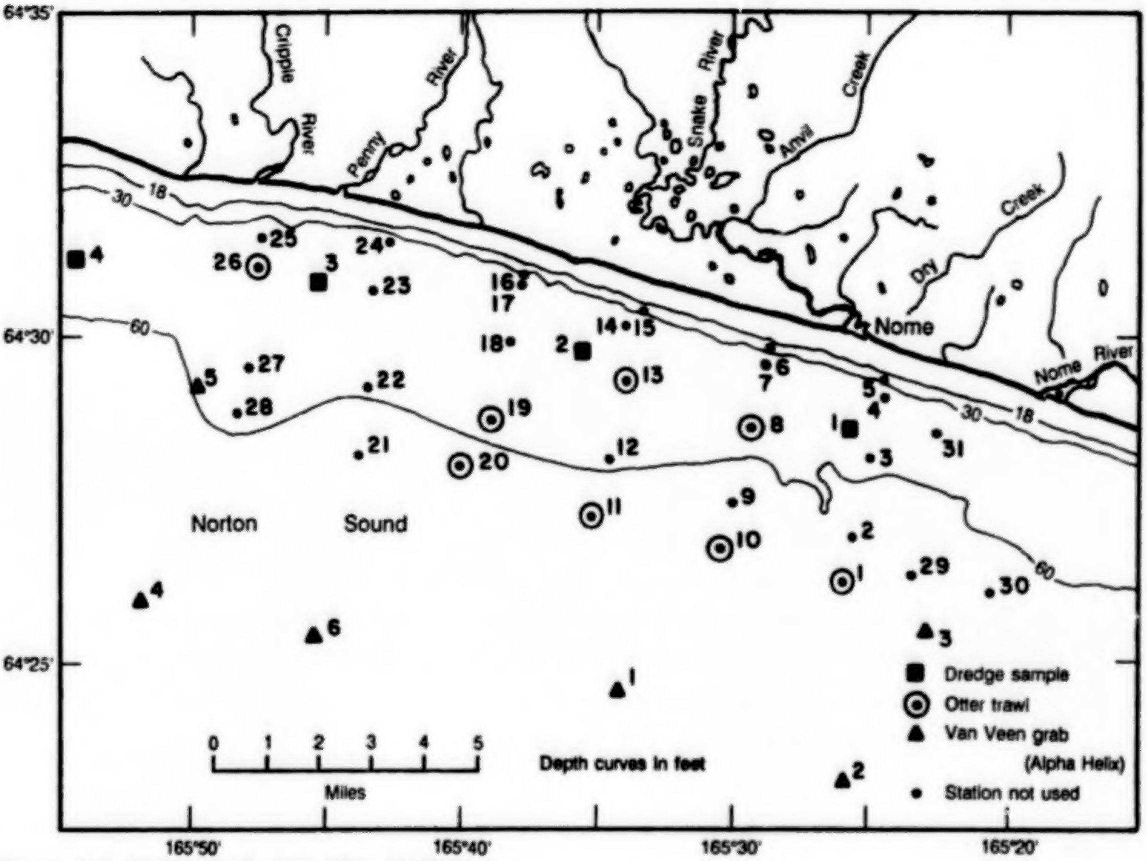


Figure 1-4. Nome study sampling stations.

In the summer of 1988 a second major study of the inshore region near Nome under sponsorship of the Minerals Management Service (Naidu *et al.* 1989). This report emphasized trace element concentrations in seawater, sediments, and selected biological tissues. It also contains data on Biological Oxygen Demand (BOD), and light transmission downstream to the gold dredge operating in state waters.

I hope that this briefing of the past oceanographic work in the Nome area will be of benefit to us in our deliberations. Any monitoring scheme devised benefits greatly from a time series of data that can be used to detect change. We are fortunate in having a limited amount of such data for our present deliberations.

It is now time to proceed with our agenda. It gives me great pleasure to introduce to you, Mr. Charles Mitchell, who is President of MBC Applied Environmental Sciences, the organization responsible for organizing and coordinating this meeting, who has some things to say about the structure of the workshop and some logistics information. He will introduce the next speaker.

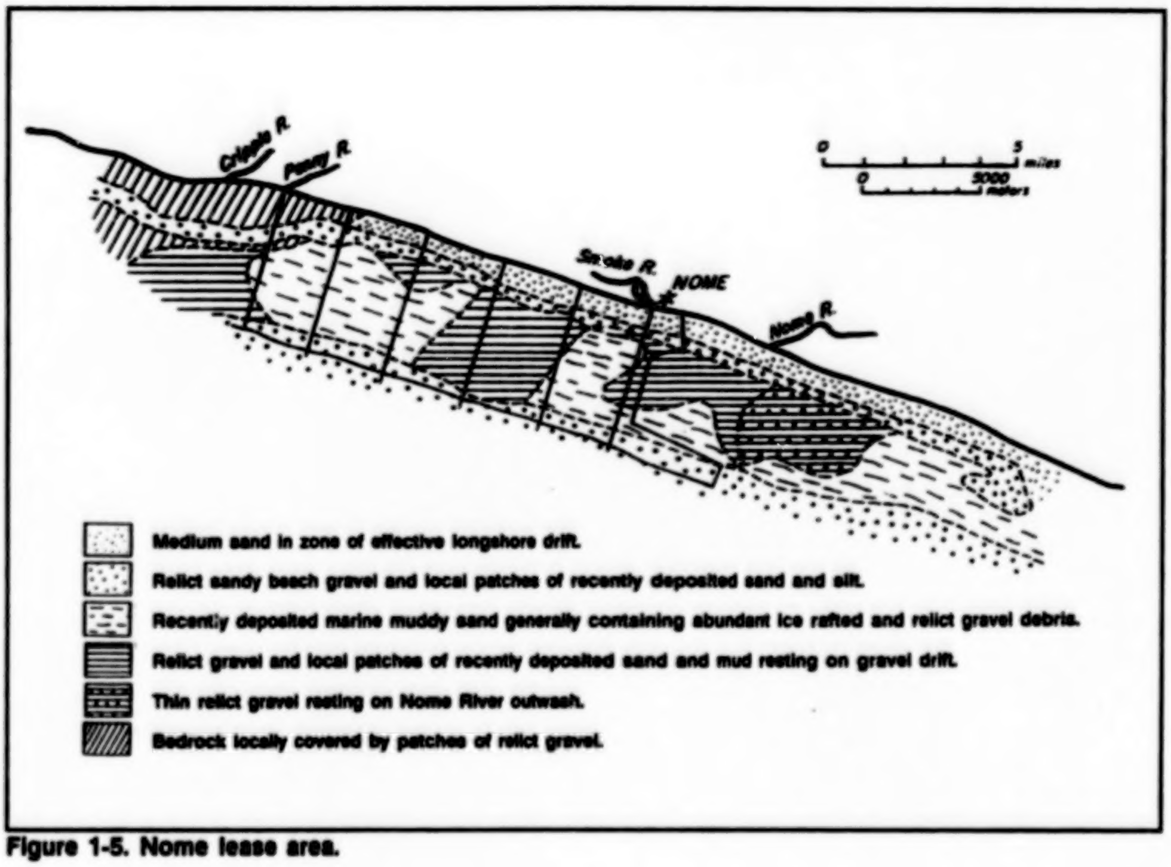

Cripple R.
Penny R.
Snake R.
NOME
Nome R.
0 5 miles
0 5000 meters
Medium sand in zone of effective longshore drift.
Relict sandy beach gravel and local patches of recently deposited sand and silt.
Recently deposited marine muddy sand generally containing abundant ice rafted and relict gravel debris.
Relict gravel and local patches of recently deposited sand and mud resting on gravel drift.
Thin relict gravel resting on Nome River outwash.
Bedrock locally covered by patches of relict gravel.

Figure 1-5. Nome lease area.

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WORKSHOP STRUCTURE, PRODUCTS, AND LOGISTICS

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Thanks Don. I have a few things to say about the structure of the workshop today and some of the logistics. The table arrangement will be maintained over the next couple of days and essentially houses the invited participants to the conference and workshop. These are the people who provide the brains that we are really going to pick over the next few days. We will be covering a variety of subjects having to do with the potential impact and the design of these monitoring programs that will provide the baseline information. We would like to keep the discussions open; we would like to keep them active. You all have bits and pieces of information that MMS needs to put this thing together by the end of the week. On the last day of the workshop we will have a slightly different configuration. We will break into four groups to help hammer out the last minute recommendations and polish them and summarize it. It is a working workshop, we expect all of you to work. We know that we have to compete with some of the other conferences that are going on this very floor. To entice you to stay we had the Hilton provide these gifts under the Christmas tree for those of you who make it through the end of the week. I'm jesting of course. So please, don't anybody take the boxes, we will probably have to pay for them.

We've asked the session chairs to provide a kind of short summary at the end of each session. We are doing this because we expect that there will be a lot of exchange of people between this workshop and some of the other workshops. People may be ducking in and ducking out so we are asking that the session chairs give a short synopsis at the restart of the meeting after each break. We are also asking that the public at large to hold your questions until the last ten minutes or so of the sessions. We will have to see how this is going to work but just so that we don't have our experts breaking away and trying to respond to the needs of other groups at this point and time but we do want to have all of your input.

Now I would like to introduce the first speaker today. Eric Crecelius from Battelle Northwest Laboratories.

RECENT RESULTS ON TRACE METAL CONTENT OF ENVIRONMENTAL SAMPLES IN VICINITY OF NOME, ALASKA

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There is concern that future gold dredging activities in federal waters of Norton Sound may mobilize metals, resulting in elevated concentrations of metals in the water column. Battelle collected water column samples at six regional stations within about 50 km of Nome in June 1989 to establish the baseline concentrations of metals (As, Cd, Cu, Hg, Ni, Pb, and Zn) in the region of Norton Sound, Alaska. Also, water samples were collected up current and down current of an active gold dredge in June and September 1989 to determine if the dredge was mobilizing metals. The concentrations of metals in the water column at the regional stations were typical of uncontaminated coastal seawater (As 1, Cd 0.1, Cu 1, Hg 0.001, Ni 0.5, Pb 0.1, and Zn 1 $\mu g/\ell$). Metal concentrations did not vary greatly with water depth or distance offshore. Concentrations in the Snake River, which flows through Nome, were also relatively low and could not have a significant effect on concentration of metals in regional seawater. The concentrations of metals in water samples collected down current of the gold dredge were similar to those at the up current and regional stations, except for water column samples that contained high concentrations of suspended sediment. When seawater samples were acidified, the suspended sediments released metals to the water sample. If turbid water samples were first filtered before acidification, then the concentrations of metals in filtered seawater were baseline, indicating the gold dredging operation was not releasing detectable concentrations of soluble metals.

PRESENTATION AND DISCUSSION

This spring Battelle Northwest was approached by MMS with the interest in having some water samples collected and analyzed from the region around Nome. There was controversy over what the actual concentration of some of the trace metals were in the area. We had some experience doing low level trace metal analysis in water and agreed to do some work in this area.

The first objective of this study was to determine the concentration of seven metals in regional seawater samples from the Nome area. We also were to determine the concentration of metals in the plume near the BIMA, the active dredge, that's in the Nome area, and the Snake River which enters Norton Sound through the city of Nome. We were also to compare the EPA Total Recoverable Digestion Procedure, Method 3005, for analyzing seawater, to the chemical oceanographic techniques which are state-of-the-art methods that are used for trace metals in seawater. EPA has required that the monitoring around the existing dredge operation be done using the EPA Total Recoverable Digestion. We are comparing our analytical techniques to the EPA digestion procedure. We also conducted a few sediment elutriate experiments. These are tests where we mixed seawater and sediment together to see if metals are being released from sediment when suspended in seawater. The last objective was to measure the concentration of mercury and arsenic in hair samples from women of childbearing age in the Nome area.

Figure 1-6 shows the area near Nome where the proposed lease sales will be held in the future. The Snake River comes through the city of Nome and actually forms a small harbor in the city of Nome. The area where the dredge was active this past season was west of Nome,

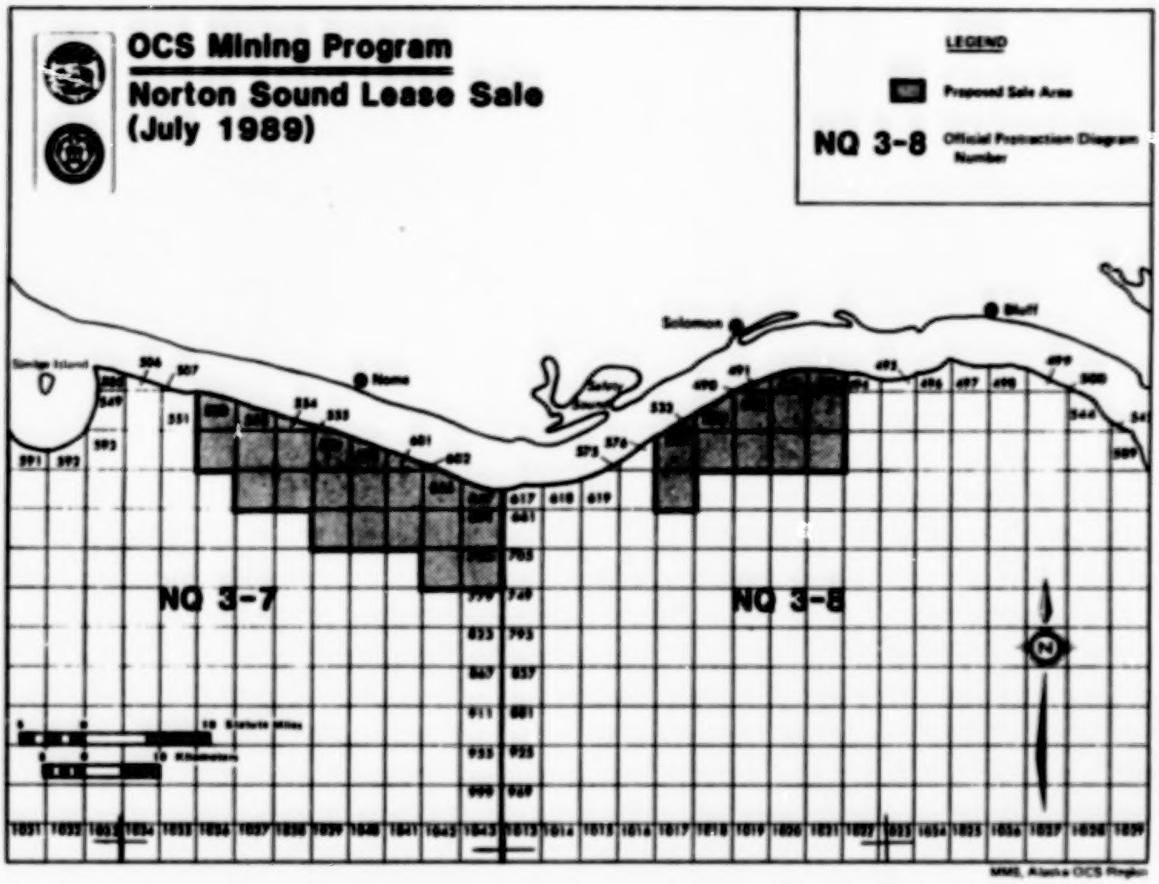


Figure 1-6. Norton Sound lease sale area.

within a couple miles of shore and a water depth of about 10 m (30 ft) approximately. The station locations shown on here were previously sampled by the University of Alaska, Fairbanks a little over a year ago. We reoccupied a few of these stations, collecting water samples. The location where we were sampling was some distance away from the active dredging area. We were sampling at least 30 miles to the east of Nome and approximately 10 miles offshore. This area is fairly shallow and uniform depth; water depths of 10 to 20 m, fairly fine grained sand and sandy/silty sediments in this area. We collected water samples near the surface and near the bottom at six stations in the middle of June. We also collected water upcurrent and downcurrent of the BIMA dredge on two different days in June when they were actively dredging. We were sampling upcurrent about 2,000 m and downcurrent about 100 m. The 100 m station is right in the visible plume of the dredge operation. Of the three elutriate sediments that we tested, two of them were collected a year before by the University of Alaska and were frozen. We analyzed sediments from Station 3 and Station 10, and also a sediment sample that was collected in mid-September of this year near the BIMA. This was a very preliminary study for elutriate tests.

I want to give you some idea of what the sampling involved. It is not the same as going to southern California where you can charter research vessels.

There are very few small boats in the town of Nome. The research vessel *Eager Beaver* has been used for monitoring programs around the BIMA dredge. We were able to use this suitable vessel for collecting water samples. If your monitoring program required heavy equipment you would have to charter a vessel from some distance away and it would become relatively expensive. Also this area is ice-covered a good percentage of the year so that's another limitation as far as using research vessels. Although the area is fairly open from the months of June, July, August, September, October.

The BIMA is quite a large structure. It's total length is about two football fields long – it's over 600 ft long. There are cables off to the sides which are used to systematically move the dredge back and forth over the bottom and accurately dredges specific areas. The cables also are a hazard or interference to sampling. Your sampling plan must stay out of the way of the cable and the dredge.

We used what I call "clean" field sampling methods. We used teflon-coated Go-Flow water sampling bottles, a plastic hydro weight and a nylon hydroline to avoid contamination of the water column. We used a plastic pulley and a plastic line spool to avoid contaminating the water with our sampling equipment. The water was placed in teflon bottles to be shipped back to the laboratory. On shipboard we did filtering with an all plastic filter unit for the determination of dissolved vs. total metal in the water column. We acidified the seawater to pH 1.8 with high purity acid before shipping.

The analytical methods that were used for the trace metal analyses included the following: mercury was analyzed by cold-vapor atomic fluorescence with a gold amalgamation preconcentration step. It has very good sensitivity for mercury in seawater, detection limit in the range of 0.1 parts per trillion (0.1 ng/ℓ). We can analyze at the part per trillion (ppt) level of mercury in seawater using a volume of seawater of 10 to 20 mℓ. We had two operationally defined methods. For the easily reducible mercury, 1) that which can be swept out of seawater by adding stannous chloride as a reducing agent, and 2) total mercury which would include mercury that is complexed with organic matter, this is destroyed by a bromine monochloride oxidation step, a strong oxidizing agent. Then the water is reduced with stannous chloride, the mercury swept out into the preconcentrator, and then into the detector. Cadmium, copper, nickel and lead were analyzed by a preconcentration step using ammonium pyrrolidine dithiocarbamate (APDC) cobalt. This material scavenges the materials out of seawater. They're collected on a filter, dissolved, and then analyzed by Zeeman graphite furnace atomic absorption (ZGFAA). Arsenic is analyzed by hydride reduction and cryogenic trapping; zinc was analyzed directly by ZGFAA. EPA total recoverable method uses a digestion, with quite a bit of acid, heats and evaporates off a lot of the water and then the water is diluted to its initial volume. This is followed by analysis by mercury, analyzed again by cold vapor atomic absorption, and then the other metals were analyzed directly by ZGFAA without preconcentration. The EPA procedure says to digest the sample with strong acid to free up the metals from the solids. This procedure is designed for analyzing wastewater, freeing the metals from the particulate matter and then analyze them directly in the graphite furnace. The sensitivity is lower because we do not go through a preconcentration step with the EPA method. We did not use the EPA mercury analysis method because the detection limit for that is much inferior to the preconcentration method. We felt we would never detect mercury in the water samples if we used the EPA method developed for wastewater. We analyzed some certified freshwater and did an intercomparision study for mercury with Gary Gill of the University of California, Santa Cruz. There is no seawater sample that is certified for mercury but for these other metals there is certified seawater that we use to check our accuracy.

Table 1-1. Concentrations (μg/ℓ) of metals in seawater from Norton Sound and other locations.

Sample	AS	Cd	Cu	Hg	Ni	Pb	Zn
Norton Sound	0.8	0.06	1.0	0.001	1.17	0.1	<0.65
Puget Sound	1.7	0.1	0.5	0.001	0.5	0.1	1.0
S.E. Alaska	1.1	0.1	0.6	0.0004	0.6	0.1	1.0
EPA Criteria	36	9.3	2.9	0.025	8.3	5.6	86

Table 1-1 is a summary of the concentration of metals in seawater of Norton Sound, and comparative areas with EPA criteria. Data in Table 1-2a are from the Norton Sound regional stations sampled in June. Again we sampled a couple of the stations in September of this year (Table 1-2b). These are the average or approximate concentrations in this area. I want to note and compare these with Puget Sound and an area in southeastern Alaska near Juneau where we have had a monitoring program for several years. Then we will look at the EPA water quality criteria. If we go down these element by element: arsenic levels in Norton Sound are somewhat lower than what would be typical for other marine areas in the Northeast Pacific. Typically I would say, in the ocean, arsenic is around 1.5 μg/ℓ. I don't have a good explanation why the Nome area seem to be consistently a little lower than that. The EPA water quality criteria which is based on arsenide or arsenic in the +3 valence state is well above what is typically found in seawater anywhere in the world. Cadmium concentrations in Norton Sound are low and typical of what are found in other areas in the Pacific; water quality criteria is nine. Copper in Norton Sound is relatively high, 1.0 μg/ℓ. We see that in other areas and out in the open ocean surface water, copper is quite a bit lower where it has been stripped out of the water column by the plankton. The EPA water quality criteria for copper is very close to the natural level in the water. You can see that in Norton Sound the copper level in the water is one third the EPA concentrations in Nome are about 0.001 μg/ℓ, that would be 1 ppt or 1 ng/ℓ. This is typical of mercury in the world oceans and similar to levels in Puget Sound, and in southeast Alaska. Water quality criteria would be 0.025 μg/ℓ or 25 ppt. The nickel in Norton Sound is up around 1.5 μg/ℓ. This is also, as in a case of copper, a very high level for nickel in seawater. I don't have a good explanation for that either. You can see in Puget Sound, in southeastern Alaska, and those coastal areas, the nickel level is about a third or half of that in Norton Sound. Lead concentrations are very low in Norton Sound as is the case in most other coastal areas. Usually the only time lead is elevated is near a major industrial source. Lead has a fairly short residence time in seawater and consequently global input of lead to the ocean is quickly removed. We have very low levels of lead. Zinc was below our detection limit; water quality criteria for zinc is 86. We are not approaching the criteria for zinc. Are there any questions on this data for the regional metals in the Norton Sound before I go on?

Don Hood: What is the variability in concentrations of trace metals with depth and regionally?

Eric Crecelius: Ok. If you would like I can show - do you want to see a slide of some of the detailed data? Ok. (Table 1-2.) Some of these numbers are very tight, in other words, the regional metals regionally might vary ±20%. Some of them vary almost 100%. Lead concentrations vary up to 0.2 μg/ℓ in the bottom water where there is higher suspended matter.

Table 1-2a. Concentrations (µg/ℓ) of trace metals in seawater collected in June 1989 from regional stations in Norton Sound near Nome, Alaska.

Station	Sample	AS	Cd	Cu	Hg	Ni	Pb	Zn
4	S-1	0.77	0.072	0.79	0.0007	1.80	0.067	<0.65
4	S-2	0.82	0.067	0.76	0.0016	2.11	0.029	<0.65
4	B-1	0.95	0.067	1.23	0.0011	2.45	0.140	1.03
4	B-2	1.01	0.069	1.11	0.0012	1.70	0.146	<0.65
5	S-1	0.81	0.063	0.76	0.0012	1.87	0.014	<0.65
5	S-2	0.74	0.064	0.77	0.0008	1.66	0.043	<0.65
5	B-1	0.77	0.074	1.16	0.0012	1.30	0.131	<0.65
5	B-2	0.86	0.071	1.18	0.0012	1.63	0.137	0.83
7	S-1	0.81	0.070	0.67	0.0011	0.82	0.025	<0.65
7	S-2	0.57	0.070	1.00	0.0017	1.55	0.020	<0.65
7	B-1	0.95	0.070	1.31	0.0019	1.49	0.260	1.14
7	B-2	0.68	0.068	1.17	0.0014	1.61	0.199	<0.65
8	S-1	0.58	0.066	0.91	0.0010	1.22	0.058	<0.65
8	S-2	0.43	0.065	0.82	0.0019	0.84	0.052	<0.65
8	B-1	0.92	0.065	1.42	0.0020	1.60	0.359	0.93
8	B-2	0.80	0.063	1.46	0.0021	1.66	0.392	1.86
9	S-1	0.62	0.052	1.60	0.0009	-	0.218	-
9	S-2	0.74	0.056	1.63	0.0008	1.35	0.072	<0.65
9	B-1	0.98	0.050	1.34	0.0015	-	0.261	-
9	B-2	0.89	0.060	1.23	0.0012	1.70	0.212	<0.65
10	S-1	0.50	0.064	0.97	0.0008	-	0.039	-
10	S-2	0.85	0.062	1.00	0.0006	-	0.026	-
10	S-3	0.84	0.051	1.10	0.0008	-	0.060	-
10	B-1	0.69	0.061	1.14	0.0013	1.71	0.205	<0.65
10	B-2	0.88	0.063	1.11	0.0012	1.81	0.198	<0.65
10	B-3	0.90	0.068	1.23	0.0015	1.92	0.185	0.83

- = Data not available.

Interlaboratory comparison for total mercury (µg/l) in seawater collected from Norton Sound in June, 1989.

Station	Sample	Date	Battelle Northwest	Gary Gill - UCSC
100 m downcurrent	Mid depth	6-19	0.0024	0.0037
9	S-1	6-19	0.0010	0.0007
10	S-3	6-20	0.0017	0.0012

These are total metals in the water and so when there's natural suspended matter, lead increases up to 0.2 or 0.3 µg/ℓ and in the surface water it's 0.05 or 0.03 µ/ℓ. There is some variability in these results.

Ray Emerson: You combined the three depths into one average for mercury?

Eric Crecelius: Yes. At regional stations we sampled surface and bottom water, about a meter under the surface and a meter above the bottom. I just lumped it together here and took an

Table 1-2b. Concentrations (μg/ℓ) of trace metals in seawater collected in September 1989 from regional stations in Norton Sound near Nome, Alaska.

Station	Sample	AS	Cd	Cu	Hg	Ni	Pb	Zn
9	S-1		0.063	0.76	0.0006		0.100	
9	S-2		0.062	1.25	0.0004		0.021	
9	B-1		0.064	0.71	0.0009		0.201	
9	B-2		0.057	0.63	0.0008		0.094	
10	S-1		0.057	1.31	0.0004		0.017	
10	S-2		0.057	1.43	0.0003		<0.007	
10	B-1		0.061	0.74	0.0007		0.116	
10	B-2		0.059	0.74	0.0007		0.081	

average. For lead there is a difference between surface and bottom water. For some of the metals there isn't any trend with depth. There doesn't seem to be any trend offshore or along shore, we could not detect any regional trends. We see a slight depth dependence on some of the metals which I think would be related to suspended matter. Although suspended load in the water column in the regional stations is quite low. The water is fairly clear. At times there can be some resuspension in the bottom sediment.

Ray Emerson: The particulate material is filtered off then?

Eric Crecelius: No. This is unfiltered water. I can show you some results later for filtered water. It made very little difference because most of these metals are in the soluble form.

Ray Emerson: So there was an acid digestion to the total sample?

Eric Crecelius: In these results for these three regions, water samples was acidified to pH 1.8. They were then analyzed either through a preconcentration step which included the particulate matters such as APDC precipitation procedure includes suspended matter. When we do total mercury, the strong oxidizing agent should free most of the mercury from suspended matter, either strip it off the clays or break down the organic complexes of mercury. Zinc is done by direct injection so that the suspended matter would be included in that analysis. So this is basically total metal in the water column at the regional stations. The surprises are that copper and nickel are relatively high. But I don't believe that it is from a local source because you can travel 50 km along the coast or go offshore 16 km or come back two months later and find basically the same concentrations, so I think this is regional. This is typical of Norton Sound in the Nome area in the summertime. It may not be typical other times of the year when there is a different circulation regime in the area. I don't know if there's any influence in this area from the Yukon River which is a large river which is 70 to a 100 miles to the southeast. I am not sure what the influence of the Yukon would have.

Ray Emerson: So when you got down to 1.2 pH...

Eric Crecelius: We stored the seawater at 1.8 pH.

Ray Emerson: You still have a relatively turbid sample I suppose?

Eric Crecelius: No. The water is essentially crystal clear in the regional stations. Now when we get into the BIMA stations, downcurrent 100 m, then we have a different scenario. Most of the metal is particulate for some of the elements and I will show you some data on that in a minute. I just want to set the stage here for what we found for the metals in the regional water. This is water that is more than five miles offshore.

Table 1-3 shows data for the trace metals in the Snake River. We sampled two stations in the mouth of the Snake River in Nome in June. This is total metal in the water column. There was a little turbidity in the Snake River. It's a fairly small river. The point here is that the Snake River would have an insignificant effect or contribution of trace metals to the dredging area. One concern, is the Snake River a major source of pollutants of metals due to past or existing mining activities in its watershed? Arsenic level is higher than in the coastal water. Cadmium is lower; copper is about the same; mercury is slightly higher but still there is not a plume of mercury coming through the Snake River. Nickel is about the same as on the coast; lead is a little higher; and zinc is a little higher. So there really aren't any surprises in the Snake River. I wouldn't be at all concerned about studying the Snake in the future. If it was discharging high levels of metals then you would want to be concerned about how that could affect a monitoring program.

Table 1-3. Concentrations (µg/ℓ) of metals in the Snake River water near Nome, Alaska, on June 20, 1989.

Station	AS	Cd	Cu	Hg	Ni	Pb	Zn
1	3.70	0.017	0.99	0.004	1.52	0.357	3.42
2	4.08	0.029	1.09	0.004	1.81	0.424	2.28

Table 1-4 shows results for water that was collected in September around the BIMA. The upper data are from filtered water collected near the BIMA. We have surface, mid-, and bottom water collected at 100 m downcurrent of the BIMA, which would be in the turbidity plume. Then 2000 m upcurrent collected on the same day or within a couple of hours of the bottom water collected 2000 m upcurrent of the BIMA. I'm sorry that I don't have all of the results in this table, we are still analyzing some of these from the September samples. I can show it from the June samples (Table 1-5). The cadmium is typical of the region, copper is not much above what I would say is the copper for the region. Mercury and lead are also the same as the region. When you remove the suspended matter, filtered 0.4 µ, we see basically regional seawater even in the turbidity plume of the BIMA. When we sampled unfiltered water, these same water samples downcurrent, surface, mid-, bottom; and upcurrent, surface, mid-, bottom, the dramatic change is that the copper concentrations go from 1 µg/ℓ to 50 µg/ℓ. The lead concentrations go from a 0.1 to 13 µg/ℓ. The upcurrent values don't really change because there is not much suspended matter regionally or upcurrent of the BIMA. So whether you filter or not filter, you don't change the concentrations of the upcurrent data. But you can see that there is a lot of copper and lead. There will also be a lot of zinc, nickel and arsenic when you collect muddy water and acidify the water and release those metals to the water. That is why we are seeing elevated metal concentrations in the unfiltered seawater. But if you filtered it first you remove the suspended matter which contains the metals in excess of what you would see regionally in the area. Are there any questions on this data or the methods or the conclusion of this?

Ray Emerson: What happened to the mercury in the unfiltered?

Table 1-4. Concentrations (µg/ℓ) of trace metals in seawater collected in September 1989 near the BIMA gold dredge in Norton Sound, Alaska.

Station	Sample	As	Cd	Cu	Hg	Ni	Pb	Zn
					Filtered			
100 m downcurrent	Surface		0.059	2.38	0.0008		0.030	
100 m downcurrent	Mid		0.019	1.67	0.0009		0.060	
100 m downcurrent	Bottom		0.028	0.50	0.0007		0.009	
2000 m upcurrent	Bottom		0.063	1.09	0.0005		<0.007	
					Unfiltered			
100 m downcurrent	Surface		0.066	1.46	0.0009		0.184	
100 m downcurrent	Mid		0.230	50.65	0.0030		12.5	
100 m downcurrent	Bottom		0.112	51.25	0.0030		13.3	
2000 m upcurrent	Surface		0.066	1.39	0.0002		0.069	
2000 m upcurrent	Mid		0.060	1.42	0.0008		0.056	
2000 m upcurrent	Bottom		0.067	1.47	0.0008		0.141	

Eric Crecelius: You can see that the mercury levels went from about 1 ppt up to about 3 ppt. What it means is when you acidify suspended sediment you will release a little mercury. The total mercury goes up when you suspend the water. If you filter it you remove the particulate mercury before you analyze. If you don't filter, and you have a lot of turbidity in the water, you'll also have more mercury in the water. Still it's low, we're talking about 3 ppt and the water quality criteria is 25. It is true for every metal, that if you suspend a lot of sediment in the water and analyze for total metal you are going to detect metal.

Table 1-5. Concentrations (µg/ℓ) of trace metals in seawater collected in June 1989 near the BIMA gold dredge in Norton Sound, Alaska.

Station	Sample	Date	As	Cd	Cu	Hg	Ni	Pb	Zn
2000 m upcurrent	Surface	6-19	0.87	0.072	1.15	0.0009	1.71	0.138	<0.65
2000 m upcurrent	Mid	6-19	-	0.071	1.38	0.0014	-	0.244	-
2000 m upcurrent	Bottom	6-19	1.77	0.071	1.51	0.0027	1.71	0.411	1.45
100 m downcurrent	Surface	6-19	-	0.065	1.57	0.0010	-	0.343	-
100 m downcurrent	Mid	6-19	-	0.110	11.3	0.0011	-	2.92	-
100 m downcurrent	Bottom	6-19	4.98	0.135	32.7	0.0014	24.4	4.20	13.04
2000 m upcurrent	Surface	6-21	1.47	0.067	1.61	0.0004	1.81	0.242	<0.65
2000 m upcurrent	Mid	6-21	1.89	0.074	1.14	0.0007	1.99	0.218	<0.65
2000 m upcurrent	Bottom	6-21	1.36	0.070	1.44	0.0007	1.81	0.228	<0.65
100 m downcurrent	Surface	6-21	1.66	0.074	1.28	0.0015	2.56	0.228	<0.65
100 m downcurrent	Mid	6-21	2.48	0.081	1.65	0.0006	2.56	0.551	1.97
100 m downcurrent	Bottom	6-21	9.17	0.127	20.6	0.0009	21.0	3.44	9.83

- = Data not available

Table 1-6 is the elutriate data. In this case, sediments were resuspended. The elutriate procedure was developed by the U.S. Army, Corps of Engineers to estimate the effects of resuspending sediment during dredging operations or dredging disposal operations. The rationale is to take receiving water, which would ordinarily be oxygenated receiving water, in this case Norton Sound water, and suspend some sediment in the receiving water for a period of

Table 1-6. Concentrations ($\mu g/\ell$) of trace metals in Norton Sound sediment elutriates (filtered).

Station	pH	As^{+3}	As^{+5}	Cu	Hg	Ni
Station 3	7.6	4.1	1.2	0.34	0.00044	
Station 10	7.6	3.4	1.2	0.48	0.00037	
BIMA Site (9-15-89)	7.6	10.9	7.1	0.56	0.00026	
Norton Sound Receiving Water	7.8	0.1	0.6	1.17	0.00019	

time (mix for about 30 minutes) then filter the water and analyze the filtrate for the dissolved metals. It is an estimate of what would be mobilized during a dredging operation or a disposal operation. We used sediment from Station 3 which was over to the east, Station 10 which is offshore from Nome about 5 to 10 miles, and the BIMA site sediment and Norton Sound receiving water. This is water that we collected offshore of Norton Sound near Station 10 in September and mixed with sediments and measured the pH. The pH of the seawater is about 7.8 which is a typical pH. When we mixed it with the sediment, the pH dropped slightly. We analyzed for arsenic (in the +3 and +5 valence state), copper and mercury. A couple of surprises here, well they are not real surprises because this has been seen in other elutriate tests. When you mix the water which is about 1 $\mu g/\ell$ Cu in this area, it absorbs copper from the water. The sediments are actually removing copper. They are probably not changing the mercury at all, possibly adding a tiny bit of mercury. These values are all below a ng/ℓ, below a part per trillion. I would not say that there is much happening there. But copper went down during the elutriate procedure. Some arsenic is released; both arsenic +3 and arsenic +5 were well below water quality criteria. But there is a little mobilization of arsenic. We don't have the nickel results yet. Are there any questions about the elutriate test? These were the only metals we are looking for in the elutriates.

Woollcott Smith: I guess I don't understand the test. The original sample is filtered?

Eric Crecelius: The test is to take one part water, I don't remember exactly the ratio, but I think it's one part sediment and five parts water, mix this up for a period of half an hour in a beaker and then filter that water and analyze for the soluble metal. This is the amount of soluble metal that was released from the sediment mixed with Norton Sound seawater. We see a little bit of arsenic being released and we see copper being removed from the water by the sediment.

Woollcott Smith: I guess the one microgram per liter is what's bothering me.

Eric Crecelius: That's one microgram per liter of the filtered elutriate.

The next data I am going to talk about is the total recoverable (Table 1-7). We haven't finished all of the analysis of the total recoverable. What we wanted to compare here was the EPA method recommended for analyzing wastewater and the methods we use for analyzing the water. At the bottom of the table I've summarized water from Stations 9 and 10. I showed you what are considered the chemical oceanographic methods. Shown here are water samples both from upcurrent and downcurrent of the BIMA and water samples from Stations 9 and 10 that were analyzed by total recoverable method. The water sample is digested with a lot of acid. At high temperature, most of the water evaporates off and it is rediluted. During this process, you are adding contaminants from this large amount of acid, plus during the evaporation procedure you get some contamination from the air that's sweeping over the container that has evaporated

Table 1-7. Concentrations (μg/ℓ) of trace metals in seawater collected in June 1989 near the BIMA gold dredge in Norton Sound, Alaska.

Station	Sample	Date	As	Cd	Cu	Hg	Ni	Pb	Zn
By total recoverable digestion, EPA Method 3005									
2000 m upcurrent	Surface	6-19		0.70	<4.5	0.021			
100 m downcurrent	Surface	6-19		<0.68	<4.5	0.019			
100 m downcurrent	Mid	6-19		<0.68	10.8	0.031			
9	S-1	6-19		<0.68	<4.5	0.020			
9	B-1	6-19		<0.68	<4.5	0.037			
10	S-1	6-19		<0.68	<4.5	0.020			
10	S-2	6-19		<0.68	<4.5	0.022			
10	S-3	6-20		0.73	<4.5	0.019			
Concentrations determined by chemical oceanographic methods.									
9 and 10	5 samples	6-19	0.7	0.056	1.20	0.0009	1.7	0.1	<0.65

off of the acid. The methods recommended by EPA aren't very compatible for a seawater-acid mixture, so you don't have a very good detection limit. So what we come up with is undetectable levels of cadmium; higher undetectable levels of copper; and the mercury values instead of being about 1.0 ng/ℓ have gone up about 20 ng/ℓ. I believe that is because this hot acid mixture is absorbing mercury out of the atmosphere. In the process we are either raising our detection limits or we are contaminating the water. I feel that it would be better to use some other types of methods on these water samples than the EPA total recoverable.

I would like to summarize the metals data for the regional seawater samples. I feel there are uniform concentrations with depth and distance offshore of the seven metals we have examined. Copper and nickel concentrations are higher than in coastal water from other areas of the United States. Other metals are typical of uncontaminated coastal seawater and the EPA total recoverable digestion method I feel is not appropriate for seawater. Looking at the summary of the BIMA dredge results, the unfiltered turbid water contains elevated concentrations of metals. This is due to metal associated with the suspended sediment. Filtered, turbid seawater samples contained little or no elevated concentrations of metals. There is a little elevation of some of these, they are still well below water quality criteria. The Snake River is not a significant source of metals to this region of Norton Sound.

The other study we were involved with was to look at the concentrations of mercury and arsenic in hair in the area. These samples were collected this fall in Nome. The concern was if high concentrations of mercury were present in women at childbearing age, their offspring would have the possibility of having some kind of biological human health effects due to high concentrations of mercury. It turns out that the mean concentrations of mercury of 200 women was 0.97 ppm ±1 SD and the range was from 0.1 to 8 ppm in hair. This would be considered relatively low concentration of mercury in human hair, which was actually a surprise. We expected that if this population had a significant dietary intake of seafood (the mercury is typically elevated in seafood compared to other food sources), we would expect a fairly high level of mercury in this population. For comparison, there is a Japanese human hair standard, a certified hair standard which we analyzed and the concentration of that is 4.4 ppm mercury. I am not trying to say that the Japanese hair standard is typical for all of Japan or that population, I am not sure exactly what it represents. The level of concern is approximately 6 ppm. This is based on several studies in areas where there has been mercury contamination from anthropogenic sources and there were studies done on human health and on children and if the level exceeds 6 ppm, that is a level of concern. We are well below that. We did have two people, that exceed 6 (6.2 and a 8 ppm). That is, there were 2 out of 200 women that had hair levels above this. I'd say that there's no indication of elevated mercury concentrations in the human food chain in Nome. Now it's true that there's a couple of people that are up near the threshold level. But generally based on this, there is not evidence of a lot of mercury getting into the diet of the people in Nome. The arsenic mean is less than 0.35 ppm. Level of concern for arsenic is approximately 1 ppm. We haven't completed analysis of all these samples. We have analyzed all the samples that had high mercury levels in hair and all those samples had low arsenic. We don't see evidence of arsenic enrichment in the water column in the area and we wouldn't expect the arsenic that is taken up in the marine food chain to get to man and be accumulated in hair because of the metabolism of seafood arsenic by man is such that very little of that arsenic is retained or accumulated in humans. Are there any questions about the hair work at this time?

Ray Emerson: What was the sample size?

Eric Crecelius: Two hundred.

____: Were those two women older women?

Eric Crecelius: I'm sorry at this time I don't have the information on who the women are or anything about the study. I was only involved in the analysis of the hair samples. Someone here at MMS either now or in the near future would be able to give you more information on how these women were selected other than age, and maybe some of them will be included in a dietary survey.

____: Would you say that a woman say around 30 years who keeps eating this seafood that at age 60 the mercury level would be higher?

Eric Crecelius: I am not an expert in this field. We do have an expert, Dr. Marsh, he is a chair of this session later in this workshop on human health and I guess it's up to Don Hood at this time if he wants to talk anymore about the hair data right now or if he wants to put that off until the human health session.

_____: How many people in your sample were close to 6 ppm?

Eric Crecelius: There were 12 women out of 200 that were about 3 ppm. We had an 8, a 6, I think maybe three in the 4's and then there would be around seven or so that were in the 3's and the rest then were less than 3.

David Marsh: Maybe I could make just a brief statement and then we need to talk about this later in the human health session. When we are told the results of analyses of hair from 200 women in childbearing age, we have to question how they were selected. Now the population at risk, if there is a population, is the Native Americans, the Eskimo who lives a traditional subsistence lifestyle, hunting, and fishing. We were told a year ago that there were Eskimos in Nome and along the coast who eat seal every week; who eat other sea mammals with high organic mercury; whc eat fish all the time. These levels are not simply possible in the typical Eskimo population. Any sampling of Eskimos with that traditional lifestyle, I believe would have to include much higher hair mercury concentrations than these demonstrated in this selection of 200. One suspects that these are people who buy their food from a supermarket rather than the traditional lifestyle. In response to the question would the hair mercury concentration be higher at age 60 than 30, in general, the answer would be yes. But I think we should discuss this at much greater length in the human health session.

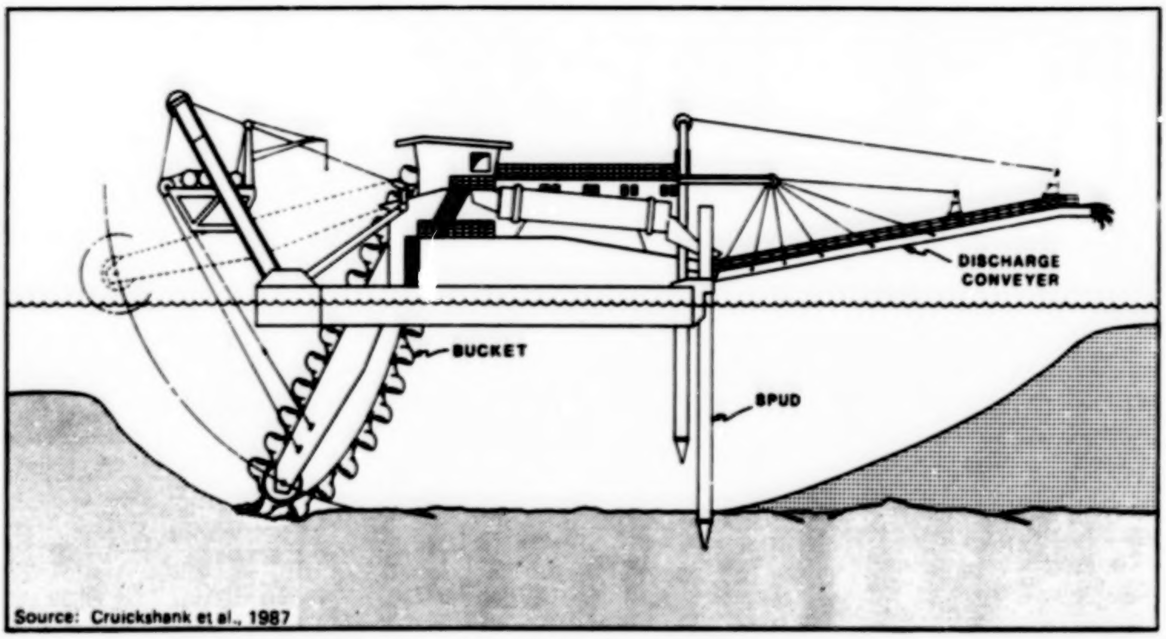
Eric Crecelius: Conclusions from this limited field study that was conducted this summer:

- The concentrations of metals in Norton Sound seawater are typical of rural coastal seawater.
- Dredging does not significantly elevate the soluble metals that are found in the plume from the dredge.
- The EPA total recoverable method is not appropriate for monitoring the seawater.
- The mercury and arsenic concentrations in the hair samples of these 200 women that were analyzed are well below the World Health Organization limit.

I am going to conclude here and I guess we can have questions. In the next session we are going to go into is basically more water and sediment.

Dick Prentki: Can you put your overhead of the trace metals behind the BIMA back on? The total numbers. The question I have is which of the trace metals of the unfiltered values are above the EPA criteria downcurrent in the dredge?

Eric Crecelius: The EPA criteria, this would be arsenic 36, cadmium, copper, mercury, nickel, lead and this would be zinc. I have them split over but you can see that for all in the BIMA here, there's two that exceed; for the total here, copper is exceeding 2.9 and lead is exceeding 5.6. I don't have data on what the suspended load was for this unfiltered water here where the copper and lead are elevated. I think it's quite possible depending upon the amount of fine grain sediment at the time and where you are dredging and the suspended load, but these values can get much higher than that, or they could be considerably lower too. But the point is that if you analyzed for total metal in a turbidity plume, you are going to exceed water quality criteria. These are not the criteria to which the WestGold dredge is presently regulated. They're regulated to a set of discharge criteria at the outlet of their pipe, at the end of the dredge where the muddy water comes out of the dredge. I'll go back to the dredging procedure. The slides that I showed you didn't show how the dredge functions. I showed you the back end of the dredge where there's a discharge of water and sediment. At the front end there is a conveyor belt with large buckets that are scooping up the sediment (Figure 1-7). Typically maybe 10 ft thickness of sediment, the dredge is bringing up these big buckets of sediment and then they're mixed with seawater onboard. The heavy minerals settle out into some type of apparatus. At the other end of the apparatus the mixture of sediment and seawater goes back into Norton Sound. The sediment settles out and the fine grain suspended matter stays in the plume for a while and dissipates. In this process of sampling downcurrent, the procedure is to sample in the turbidity plume right near the dredge. For their present monitoring program, they sample the influent and effluent from the dredge and they are regulated on the concentrations of metals in that.



Source: Cruickshank et al., 1987

Figure 1-7. Bucket ladder dredge.

I don't think it appropriate to analyze unfiltered water for total metal and then compare it to water quality criteria. Because the water quality criteria were based on different types of bioassays on marine organisms using soluble metal. If you were doing a copper bioassay to establish copper water quality criteria, you would test marine plants and marine animals to different concentrations of soluble copper and determine that toxicity. Then from that extrapolate or in someway come up with a criteria like 2.9. It's not fair then to turn around and suspend some sediment and these sediments are basically normal crustal abundance of elements, to suspend some clean sediment and then leach the metal off with acid, and then compare total metal with the water quality criteria.

_____: Do you have a sense of whether EPA would agree with you?

Eric Crecelius: I know that there are certain scientists in EPA who would agree with me. Those aren't necessarily the scientists that write regulations for discharges. So EPA has to take the standpoint of what they feel is being conservative and protecting the environment. They also may have laws somewhere that may say "you will regulate based on total metal using the EPA approved digestion procedure for waste water" and so that would be their criteria for using total recoverable metal such as this for evaluating the environmental effects.

_____: I guess my question is that if you talked with the people at EPA, would they accept the validity of this data or would they say that you can't do that?

Eric Crecelius: I haven't gone to EPA with this data and I don't think anyone else has yet. With this new data, I don't think anyone has gone to EPA and sat down with them and talked about how they would interpret this. I am not in a position to speak for EPA. I would be glad to sit down with EPA. I'm sure that MMS and WestGold Mining Company would be glad to sit and talk to them about what is additionally known now about concentration of metals dissolved and in particulates, and how you might interpret this in terms of formulating regulations in the future for proposed offshore dredging operations.

Ray Emerson: The basis for bioavailability sometimes depends on the feeding strategy of the organisms. Just because it is bound to that particulate doesn't mean that it is going to be biologically available. For example, that particulate material goes through the digestive tract of an organism, then some of it certainly is going to be taken up, be bioaccumulated at that trophic level. But, there probably is not a clear cut conclusion. What the "acute criteria" show as a number, and what we are seeing, and what you see in a number that is associated with some of these particulates ... (are different). Within that realm metals are going into the biological system – organics associated with the inorganic particulates are digested off, in many cases from bacteria which is a major food source of polychaetes, bivalves, and that sort of thing. That is their food source.

So, that is part of what EPA is talking about, I think, when they talk about the metals associated with particulates and acid digestion; they are getting at that problem. It is not an easy problem to pinpoint and in many cases it is species-specific; it is important and depends on the life history or the feeding strategy of that particular marine organism. We don't have a clear cut, yes or no answer on the acute bioassay number which is generated in the laboratory.

Eric Crecelius: I agree with you that some of the metals associated with a fine grain particulate matter is no doubt bioavailable. Biologists and chemists have not yet come up with the right digestion or extraction procedure to estimate the bioavailable metals associated with sediments or suspended matter. There have been different attempts made, different types of extractions, to try to simulate what happens in the gut of an organism. I think that treating it with strong acid is certainly overestimating the amount of metal that is there and not doing any type of extraction might be underestimating the amount of metal that is there.

CHAPTER 2

WATER QUALITY AND SEDIMENT CHEMISTRY SESSION

Eric A. Crecelius, Ph.D., Session Chair
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DETECTING CONTAMINATION OR TRENDS IN THE CONCENTRATION OF TRACE METALS ON MARINE ENVIRONMENTS

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INTRODUCTION

Three marine monitoring programs now in progress were designed to detect contaminated marine ecosystems and to qualify temporal trends in contaminant concentrations. These programs are the National Status and Trends "Mussel Watch" Program, which is funded by NOAA's Ocean Assessment Division, and the Beaufort Sea Monitoring Program, which are both funded by the U.S. Department of Interior Minerals Management Service. As part of these monitoring programs, replicate samples of sediments and organisms from numerous stations are collected annually and analyzed for contaminants.

FIELD SAMPLING METHODS

Sampling Rationale

Critical to the success of monitoring programs is the minimization of sampling variability. The sampling strategy was developed to effectively achieve the following:

- ensure uniformity of sampling techniques through establishment of and adherence to specific detailed field protocols;
- collect organisms from indigenous populations in areas considered integrative of contaminant inputs;
- collect undisturbed, depositional surface sediments from areas considered integrative of contaminants inputs;
- employ collection methods that minimize contamination;
- employ sample position-fixing techniques accurate to ±100 m or better;
- sample for auxiliary parameters (shell length, sediment grain size, organic carbon) that may be used to normalize the variability of analytical data;
- plan subsequent collections during the same season and from precisely the same site coordinates.

Sediment Sampling Methods

Sediment was collected at three or more replicate stations within a site using a Kynar-coated grab sampler or box corer designed to collect undisturbed surficial sediment. Prior to subsampling the surface sediment, the quality and integrity of the sample was determined according to specified criteria. A grab sample was acceptable if it contained overlying water (siphoned prior to subsampling), and was not acceptable if the sampler over-penetrated the sediment. A Kynar-coated stainless steel sediment scoop was specifically designed to collect uncontaminated, undisturbed sediment from the grab sampler.

Organism Sampling Methods

In sampling for bivalves, the primary objective was to obtain three discrete samples from three stations within each site, representing site replicates. However, at sites where this was not possible, a pool of bivalves representative of the site also constituted an acceptable sample. Pooled site samples or composites were generated when bivalves were collected subtidally or when the distribution of intertidal populations did not permit discrete samples to be collected. Due to the low abundances of bivalves in some areas, alternate species, such as amphipods, crabs, and gastropods, were collected in baited epoxy-coated commercial steel minnow traps.

ANALYTICAL METHODS

Analytical Rationale

The success of monitoring programs depends equally on both the design and execution of the analytical effort. The goal of the analytical program is to provide data of the highest quality via state-of-the-art techniques, provide documentation of the quality attainable (accuracy and precision), and improve the sensitivity of trend assessment by minimizing analytical variability. Based on these goals, the analytical strategy included the following objectives:

- ensure interlaboratory comparability of analytical techniques and provide method validation through participation in intercalibration exercises;
- ensure interlaboratory analytical uniformity through specific establishment of and adherence to detailed analytical protocols;
- document the quality of the data generated (based on observed limits of detection, precision, and accuracy) through adherence to detailed quality assurance protocols;
- measure auxiliary parameters such as shell length in bivalves, grain size, and total organic carbon in sediments in an effort to normalize contaminant data, correcting for variability resulting from biological, physicochemical, and geochemical processes.

Sediment Analysis

Sediment samples were freeze-dried and blended in a Spex mixer-mill, then 4 g were ground in a Spex ceramic ball mill. A 0.5-g aliquot of ground sediment was pressed into a 2-cm-diameter pellet and analyzed by energy dispersive x-ray fluorescence (XRF) (Nielson and Sanders 1983) for Al, As, Ba, Cr, Cu, Fe, Mn, Ni, Pb, Si, V, and Zn. For the metals analyzed by atomic absorption, 0.2-g aliquots of the dry homogenate were digested with 4:1 nitric acid/perchloric acid in Teflon digestion bombs in an oven for 4 hours. After these samples were allowed to cool, hydrofluoric acid was added and the digestion bombs were returned to the 130°C oven for 8 to 12 hours. The next day, boric acid was added to the solutions and the bombs were returned to the 130°C oven for 8 hours. After cooling, solution volumes were calculated and the solutions stored in polyethylene vials until analyzed. Mercury was analyzed by cold vapor atomic absorption similar to the method of Bloom and Crecelius (1983). The other metals (Ag, Cd, Sb, Se, Sn and Tl) were analyzed by Zeeman graphite furnace.

Tissue Analysis

All samples were sized and shucked prior to tissue processing. Tissue samples were freeze-dried to a constant weight and ground to a powder in a plastic mixer-mill. Half-gram aliquots of dry tissue homogenate from each station sample were reserved for XRF analysis for As, Cu, Fe, Mn, Se, Si, and Zn. For all other analysis, half-gram aliquots of dry tissue homogenate from each station sample were weighted in an acid-cleaned, preweighed Teflon digestion bomb. These samples were predigested at 50°C for 4 hours without the bomb sealed, using 4:1 nitric acid/perchloric acid, then sealed and digested at 130°C for 4 hours. After cooling, samples were diluted with deionized-distilled water. Solution volumes were calculated and the sample solutions transferred to polyethylene vials for analysis. The analytical methods used to determine specific metals included cold vapor atomic absorption for Hg, hydride for Sn, and Zeeman graphite furnace for Ag, Al, Ba, Cd, Cr, Ni, Pb, Sb, and Tl.

RESULTS AND DISCUSSION

Determining the intrasite variability for specific chemical parameters is a key step in the process of evaluating the incremental change that one can expect to detect at a given site. The determined variability includes both the intrasite or sampling variability and the analytical variability.

The intrasite variability for trace metals in sediment usually has a coefficient of variation (CV) from 5% to 20%. However, the variability occasionally exceeds 50%. The variability appears to be primarily related to the intrasite variability in grain size, and secondarily related to analytical detection limits for some elements.

Table 2-1. Minimum number of replicates necessary to detect a K-fold difference in geometric means with 80% confidence, (2-tailed t-test, x=0.05). Coefficient of variation of measurement (analytical plus sampling variability).

K[a]	0.1	0.2	0.3	0.4	0.5
1.1	19	71	157	278	433
1.2	6	20	44	77	120
1.3	4	11	22	38	58
1.4	3	7	14	24	36
1.5	3	5	10	17	25
1.6	3	5	8	13	19
1.7	3	4	7	10	15
1.8	2	4	6	9	13
1.9	2	3	5	8	11
2.0	2	3	5	7	10

[a] 1.4=40% change in value; 2.0=100% change (i.e., twofold change in value, etc).

Measurements of mud content (silt plus clay) at several sites had a CV of greater than 100%. Usually, trace metal CVs at these sites were also very high, in the range of 40% to 80%. This relationship between the high CVs for mud and trace metals is not surprising because of the positive correlation of these metals with fine grain sediments.

The concentrations of the crustal elements (Al, Fe, Mn, and Si) are less influenced by grain-size variations than are the trace elements and, therefore, had the lowest intrasite CVs, usually less than the 10%.

The intrasite CV has a direct effect on the ability to detect either temporal trends or between-site differences. Statistical calculations (Table 2-1) indicate that with a site replicate sample number of 3 and an intrasite CV of 0.1, a 1.4-fold difference in concentration would be detectable with 80% confidence using a two-sided t-test at the 0.05 significance level.

Intrasite variability for metals in bivalves is similar to that for sediments. Commonly, CV values for metals range from 10% to 20%, indicating that relatively small incremental changes in metal concentrations can be detected. For example, a 1.4-fold or 40% change can be detected if four site replicate samples have a CV of 0.1 (Table 2-1).

The temporal trends in Ba concentrations in California shelf sediments have been examined. Barium was analyzed at 13 stations both in October 1986 and January 1987. A paired t-test was used to test if the 13 station Ba means were different in October than in January. The means (724 and 738 μg/g Ba) were not significantly different at an $a=0.05$. Assuming the variance in these data is a good estimate for future differences between the 13 stations, then an absolute difference of about 25 μg/g Ba, or a 4% change in Ba, could be detected with an $a=0.01$. The power of the test used was $p=0.95$, or the probability that we reject the null hypothesis when we should.

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PRESENTATION AND DISCUSSION

The objective I see of this session on water and sediment chemistry is to come up with baseline and monitoring programs for MMS. How are they going to establish what the baseline concentrations are in Norton Sound, in order that they can assess whether changes are occurring as a result of mining activity? The goal I see for our work for this workshop is to come up with ways of establishing trends.

If trends are going to occur in the environment, they may be so small that we cannot quantify them, but we want to make the best effort we can to establish trends. We also want to select some indicator chemicals that we expect are going to change or show trends in the environment. There are a lot of chemicals out there we could monitor, but many of these are not going to change significantly. So we want to key in on those that we expect are good indicators of change. We also need to identify sites and types of sample locations, and we need to provide field and laboratory procedures that will be useful in the long term in looking at trends in this area.

I am going to go through a few of the viewgraphs of what I think are important points we need to discuss during this session during the next few hours. Then we want to get an involvement of the group and get your participation in discussing your concerns and what you feel are ways we can establish trends, select chemicals, and select sites and methods. Some of the other sessions following this one, on biology and human health, will have to address these same issues. We will come up with some of these issues again. Maybe we should put off most of the statistical concerns, such as the number of replicate samples for the water column or sediment until the statistical session, which is the last session in this series. Should we discuss that during the next few hours, or should we wait and concentrate on the number of replicates in that discussion?

One of the things we need to keep in mind are the three kinds of processes that control the sources and fates of chemicals in this area: the biological, geological and physical processes. We need to keep in mind how these are going to affect the behavior of chemicals that are naturally moving through the system and also the chemicals that may be either mobilized from the natural system or added by dredging activities. We want to be able to predict the flux of chemicals that would be coming from the dredge operation, at least as far as the sediment and water column chemistry goes. We are assuming that the dredging operation is going to mobilize some materials or maybe add some through their operation. We want to make sure we pick those to be affected by the natural processes in the area. We do have baseline data available for the concentrations of some chemicals in this ecosystem.

Earlier I talked about the data we have on the metals in the water column. There is a fair amount of data on the metals in the sediment. There's some limited data on some metals in tissues that have been sampled in Norton Sound by a number of different projects. There's some hydrocarbon data in the sediments for alkanes and aromatic hydrocarbons. This is work that was done earlier under the offshore monitoring program. It was primarily, I think at that time, directed to the possibility of exploration and drilling for oil and gas in Norton Sound and other regions of the Bering Sea.

We have been talking about metals so far, and we need to select those metals to emphasize. We also need to think again about organics. Are there certain organic compounds

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we should be monitoring? There are some conventional parameters such as grain size and total organic carbon content of the sediments that should be measured. Some crustal elements aren't going to be significantly modified by dredging activities, but they're good tracers of mineral phases and they're useful for normalizing other parameters such as iron and aluminum content of the sediments.

Another thing we need to keep in mind or get into perspective is what quantity of metal or contaminant is likely to be mobilized by a dredge and how that compares with the natural movement of this metal through this ecosystem. I have an example here I want to go through to emphasize this point. This is the hypothetical flux of arsenic from a dredge and the existing flux of arsenic from nature. In this example, we're going to say a dredge operating 100 days a year in Norton Sound is going to dredge an area 3.6 m deep by 100 acres in area. This was in the EIS, so that's where I came up with these numbers. This amounts to 1.4 million m^3 of sediment per year. All of this will go through the dredge, be mixed with seawater, and discharged out the other end. The amount of seawater that goes through the dredge is $2x10^{10}$ liters per year. This also came out of the EIS.

I'm sure that we can broaden the discussion, i.e., the depth of dredging, how deep and how big an area, and whether dredging is more or less than 100 days a year, but just for this exercise I've arbitrarily picked some parameters. We have $2x10^{10}$ liters of seawater going through the dredge. If we just looked at the total suspended solids in the discharge from the dredge, 1.4 million m^3 or 1.4 million metric tons ($1.4x10^{12}$g) - this assumes that a cubic meter of sediment is roughly a metric ton – divide that by the amount of seawater going through ($2.0x10^{10}\ell$/yr) we come up with 70 g/ℓ as the total suspended solids in the effluent on the average. But the flux from the dredge of arsenic – from the previous measurements made near the BIMA and from the elutriate test - were 4 μg/ℓ of arsenic added to the water. The natural baseline level of arsenic in Norton Sound is a little less than 1 μg/ℓ. We are then adding 4 μg/ℓ to this 1 μg/ℓ. Thus, 5 μg\ℓ of arsenic is in the effluent of the dredge for a total of 100 kg/yr of which 80 kg/yr of arsenic are added to the water column by the dredge annually. This is dissolved arsenic, arsenic that would pass through a 0.4 μ filter. Based on 100 working days per year operation schedule, this amounts to 0.8 kg soluble arsenic added to the water column each day of operation.

We now need to look at the natural advection through this area – the seawater that's naturally moving past the dredge - and estimate the amount of arsenic moving past the dredge due to natural processes. If we assume that the water column is 10 m deep, with a width of 100 m – the water column is like a river moving by the dredge with a current velocity estimated at 10 cm/sec. Now because of the tidal currents in the area, part of the day there may be almost no current or the current's moving to the east. The predominant current in the summer is to the west and frequently during maximum current flow the current exceeds 50 cm/sec. If we use 10 cm/sec on the average, that means that 8.6×10^9 liters per day of water is moved by the dredge, in a column 100 m wide and 10 m deep, or a flux of arsenic, past the dredge of 8.6 kg/day. This mass of arsenic moving past is due to natural processes. For comparison, the dredge would be adding 0.8 kg per day of arsenic. So the dredge is increasing the arsenic concentration in that water parcel by only 10%, which is almost undetectable by normal chemical sampling methods.

This calculation may be used with other contaminants and other assumptions. One can then determine the likelihood of being able to detect a change, as in this case in the water column.

If you went through a scenario of change of concentration of metal in the sediment – I haven't tried this, because there is such a small amount of the metal on the sediment that actually dissolves when the sediment is resuspended – it is probably impossible to find a change in the absolute concentration of a metal in the sediment due to a dredging activity. You are going to move the metal around because you are going to move the sediment. You are going to mix it up and you're going to resuspend it and deposit it. Then it is going to get redistributed by bottom currents. But I don't think you will be changing the concentrations of the metal in the sediment.

WATER COLUMN AND SEDIMENT CHEMISTRY SESSION

RECOMMENDATIONS

WATER COLUMN AND SEDIMENT CHEMISTRY SESSION

SUMMARY OF RECOMMENDED STUDY ELEMENTS

Baseline Program (Year 1)

All sampling from an oceanographic vessel equipped with GPS navigation.

I. Sediment

A. Sampling and Storage

1. Grabs (10 cm undisturbed)
2. 15 sites (6 in region and 3 outside the lease area)
    a. 4 in "silt"
    b. 4 in "sand/gravel"
    c. 4 in "cobble" (2 mm sieve to separate finer fraction for analysis)
    d. 2 outside lease area to east and west in similar water depth (>10 km from lease area)
    e. 1 outside lease area in deeper waters (>10 km from lease area)
    f. +2 sets of triplicates
    g. N = 17
3. Subsampling/storage
    a. 0-2 cm
    b. Grain size (50 g @ 4°C in plastic)
    c. Metals (10 g @ 4°C in plastic)
    d. Organics (N=1 (100 g frozen in glass) fine sediment)

B. Analyses

1. Grain size (% gravel, sand, silt, clay)
2. Metals (Total Concentration)
    a. Fe, Al, Mn (N=17)
    b. As, Cd, Cu, Hg, Ni, Pb (N=17)
    c. Ag, Cr, Sb, Se, Tl, Be, Sn, Zn (N=2) (1 fine, 1 coarse) (ONLY during YEAR 1)
3. Organics (ONLY during YEAR 1)
    EPA Priority Pollutants (DDT, PAH, Pesticides, PCB, Aromatic Hydrocarbons)
    N = 1 "fine" grained sample

II. Water Column

A. Sampling and Storage

1. Non-Contaminating samplers
    a. GO-FLOW
    b. Pump/Teflon tubing
2. 2 lease area sites, each over a tidal cycle
    a. 4 samples/tidal cycle
    b. 2 depths
    c. 2 sets triplicates
3. 3 regional sites (same as sediment sites)
    a. 1 sample per site
    b. 2 depths

4. Total water samples N = 26
5. Store unfiltered sample for total metals (acidify to pH <2)
6. Filtration for TSS and Particulate metals
   a. Filter within 6-8 hr
   b. 0.4 μm filters
   c. Rinse w/ pH 8 DDW (distilled, deionized water)
7. Obtain temperature, salinity and turbidity profile during each sampling period (i.e., 4/tidal cycle). Also make measurements of water current direction and velocity.

B. Analyses

1. TSS, gravimetrically (± 10 μg)
2. Total metals (acidified to pH <2)
3. Particulate metals (total concentrations in μg metal/g suspended matter)
   a. Fe, Al, Mn
   b. As, Cd, Cu, Hg, Ni, Pb

III. Interstitial Water

A. Sampling and Storage

1. 9 sites (3/region, 2 coarse, 2 fine, 2 cobble, 3 regional)
2. 2 triplicates (1 fine, 1 coarse)
3. N = 13
4. Undisturbed 0-5 cm
5. Squeezed w/in 8 hr (w/ proper pre-storage in N_2)

B. Analyses

1. Mn^{2+}, Fe^{2+}, SO_4^{-2}, Cl^-
2. As, Cd, Cu, Hg, Ni, Pb

IV. Quality Control

A. SRM's

1. Sediment
2. Seawater
3. Suspended sediment (10 mg aliquot)

B. Replicates (as specified in I-III)

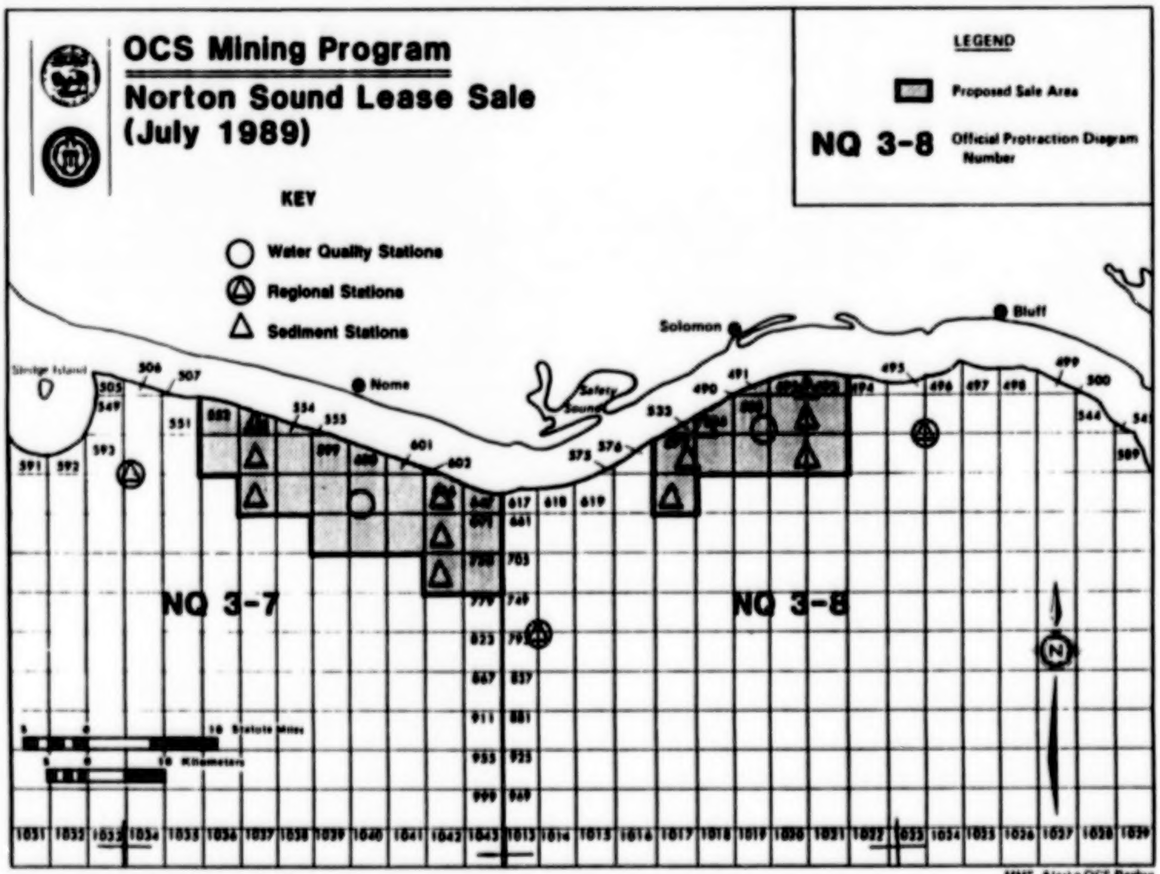
WATER QUALITY AND SEDIMENT CHEMISTRY SESSION

REVIEW AND RECOMMENDATIONS

Eric Crecelius: I will present the recommendations of the sediment and water quality group. I will go through them briefly and then we can discuss them.

STATION LOCATION

Figure 2-1 indicates the location of the proposed sampling stations. A total of 17 sites will be sampled.



MMS, Alaska OCS Region

Figure 2-1. Station locations.

Those indicated by the open circles are designated 'regional stations' and would provide coverage offshore, and 'upstream and downstream' of the lease blocks. It is anticipated that they would be located at sites where benthic ecology and chemistry studies were conducted in the early 1970s. If these sites are suitable, prior data would then be available to evaluate what, if any, changes have occurred during the last two decades.

NAVIGATION AND POSITIONING

All sampling would be accomplished from an oceanographic vessel equipped with state-of-the-art navigation to accurately locate or relocate positions. In the future it may not be necessary to use such a vessel to repeat some of the measurements, it is however desirable since it would make many of the measurements more easily accomplished.

SEDIMENT SAMPLING

Within the two lease block areas there are a total of 15 sampling sites for sediment characterization. Stations within the two lease block areas are aligned onshore-offshore. The location of stations is based on water depth and anticipated sediment grain size distribution. Inshore shallower stations would probably be characterized by a cobble or gravel substrate; the intermediate stations by sand; and the offshore stations by finer grained, silty sediments. This will result in 4 samples each for silt, sand/gravel, and cobble sediments. The cobble sediments will be screened through a 2 mm sieve and the retained fines subjected to chemical analyses. Three additional sites outside the lease area will be sampled. Two will be located east and west of the lease site but at similar depth, and one offshore in deeper water. These stations should be at least 10 km from the lease area. At two of these stations triplicate samples will be collected to provide a measure of "within station" variability.

Sediments would be collected by box corer, or using a smaller vessel with a grab that is capable of sampling sediments to a depth of 10 cm, to obtain relatively undisturbed sediments. A total of six sites from each of the lease block areas, and the three regional sites are proposed.

The upper 2 cm of collected sediment will be subjected to analysis. This layer is the most likely to be impacted, and is the area containing most of the biological activity. If a thicker layer is sampled there is a risk of diluting the signal, and if a thinner layer is sampled there may not be enough material to perform the required analyses.

A 50 g sample of sediments will be collected for grain size analyses and stored at 4°C in plastic containers. Grain size will be reported in percent gravel, sand, silt, and clay.

A 10 g sample of sediments will be retained at 4°C in plastic containers for metal analyses. Three crustal metals (iron, aluminum, and manganese) will be analyzed for total metal, i.e., the total amount after the sediments have been completely digested by HF, or been analyzed by neutron activation or X-ray fluorescence. Therefore the results would not be affected by partial recovery of the metals and the sediments. In addition, analyses for arsenic, cadmium, copper, mercury, nickel and lead will have priority.

There is also a larger group of metals included on the EPA Priority Pollutant list. Analysis for these additional metals (silver, chromium, antimony, selenium, thallium, beryllium, tin, and zinc) would be conducted on a representative fine grained and coarse grained sediment sample once during the first year of sampling at the regional stations to establish baseline conditions.

If, at a later date, changes were noted at a site, regional stations could be resampled to determine if baseline conditions had changed.

A single site with fine grained sediments will be sampled for detailed analyses of organic compounds to document background levels of aromatic hydrocarbons, PCBs, pesticides and PAHs. These are the only organic analyses proposed for sediments.

Sampling Frequency

All of the sites would be sampled at least once a year, hopefully, they could be repeated for several years before actual development or dredging occurred.

WATER QUALITY

Water quality stations would be at the same locations as the sediment samples. Samples from these sites would be analyzed to characterize dissolved and particulate metals.

It is anticipated that water quality sampling would be done during the same cruise as the sediment sampling. At each station water column profiles of temperature, salinity and turbidity would be recorded, and water samples collected from near-surface and near-bottom. Current speed and direction will be determined during the sampling period.

Non-contaminating sampling methods such as Go-flo water sampling bottles or a Teflon pumping system should be used.

Sampling Frequency

Sampling frequency will be the same as for sediment sampling, but in addition at one station within each of the two lease block areas samples would be collected at 4 hour intervals over a 25 hour tidal cycle. Triplicate samples should be collected at each of these sites during one sampling period to allow a measurement of the variability of the parameters in the water column. It is recommended that these be collected in the same water depth as at the regional sites.

Water Quality Analyses

All water samples will be processed for total suspended solids and total metals concentration for the same suite of metals as the sediments. Water would be separated into filtered and unfiltered subsamples then acidified to pH-2.

Filtered samples allow the removal of the particulates to provide analyses of suspended solids, and metals associated with particulates. Samples should be filtered within 6 to 8 hrs of collection using a 0.4 μm filter. The filters should be rinsed with pH-8 deionized distilled water to remove any salts before the determination of suspended solids by weight. The same filters would be utilized for metals analyses.

Aliquots of unfiltered water will be analyzed for metals. The analysis of total metals in both water and suspended solids allows the determination of the amounts of the target metals associated with the particulates. It also avoids the potential contamination of samples during the filtration process.

INTERSTITIAL WATER

Interstitial water is mobilized during the dredging process and diluted more than 1000 times when entrained into the materials and seawater passing through the system. Even with heavily contaminated pore water it is unlikely that it would be detectable in the receiving waters at such dilutions.

However, if a biological impact from the mobilization of interstitial water is possible it will most likely be in the surface sediments. Therefore analyses will be restricted to the upper 5 cm of undisturbed sediment cores taken at 3 stations within each lease block area, and at the 3 regional stations. In addition, triplicates samples will be taken at two sites to allow a measurement of the variability of the parameters.

Samples should be squeezed within 8 hr of collection with a nitrogen pore water squeezing device.

Pore water would be analyzed for manganese, iron, sulfate, and chloride. The manganese and iron will provide information on the redox conditions in the environment, the sulfate the sulfur reduction, and the chloride to obtain a measure of the salinity. Additionally concentrations of arsenic, cadmium, copper, mercury, nickel, and lead would be determined.

QUALITY CONTROL

Quality control would include analyzing standard reference samples or fabricating reference blanks with known amounts of constituents. Replicates would be field replicates, that is, individual samples collected over a brief period of time at a sediment or water quality station.

This represents a "bare bones" type of baseline survey and could be made more elaborate. However we have attempted to keep the ship time and chemical analyses cost minimal.

I think at this time I'll stop and we'll open this up for discussion.

Sathy Naidu: What kind of sampling device would you adopt to run analysis of metals on sediments, to avoid or minimize contamination by any of these devices that we have now?

Eric Crecelius: The type of sediment sampling equipment that is acceptable, would be a box corer or modified VanVeen grab sampler. I'm not aware of any problems with sediment contamination using clean stainless steel sediment sampling equipment. I'm not aware of any problems that have occurred in MMS monitoring programs in California or Alaska. The NOAA Status and Trends program uses a Kynar-coated stainless steel grab sampler. Kynar is a similar compound to Teflon but it's much harder and is suitable for both trace organics and trace metals.

Sathy Naidu: I suggest that you consider also the possibility of using a Haps corer.

Eric Crecelius: Right, I agree that would be a suitable sampling device. It samples relatively undisturbed sediment for at least, I think, 10 cm, is that right?

Sathy Naidu: The other thing is in the analysis of metals in the interstitial waters. Would you go through a pre-concentration process? Or would you have an adequate amount of samples to run an analysis directly on the interstitial water?

Eric Crecelius: We're not specifying what method to use to analyze the water. But I think in most cases, both the water column and the interstitial water would have to go through a pre-concentration step. We didn't put down detection limits, but the detection limits that will be required are those that will detect metals at the natural level in seawater. So we're dealing here, for most of the metals, with well below 1 ppb, (1 μg/ℓ) detection limit. And in the case of mercury you need to have a detection limit of about 1 ppt, (1.0 ng/ℓ). And it's possible to do that with water samples of several hundred milliliters. You could scale it down and probably do it on pore water with 50 ml of pore water.

Ray Emerson: What volume of water did you think you'd have to filter to get reliable numbers for your particulate suspended matter? If you're basing that on -- I thought you said the weight of the filter?

Eric Crecelius: Right, you would have to filter maybe 2 to 10 liters of water through a 47 mm filter to get a good enough weight, and to get enough particulate matter that you'd have a fairly good signal above your blank.

Ray Emerson: Was there any concern given to a need for determining the soluble or particulate organic carbons, by wet oxidation or some routine method like that?

Eric Crecelius: No, we haven't proposed any organic carbon measurements, either in the water or the sediment.

Sathy Naidu: Would you be running heavy metals on the readily mobilized fractions of sediments, rather than the gross sediments?

Eric Crecelius: Well, we've discussed that, and the main objection to that in the baseline survey would be that those techniques, when they're done by different laboratories, generate somewhat different results. And the only values you can certify are total metal. So if we specify any technique other than total metal, we'll have no way of documenting the reliability or the accuracy of that data.

It may be that some extraction of the sediment would have more significance as far as bioavailability goes. I think there's a lot of controversy over what extraction to use, what pH, and over what time period and temperature. But even if you came up with an extraction procedure, we have no way of analyzing a certified material for weakly extractable metals.

I know you and others have done some work on this in different parts of Alaska, and some of that data is available. And it might make sense to go back to some of these sites some time in the future and repeat those analyses. But one of the problems will be trying to compare results, because you don't have a standard that you can repeatedly analyze by that method. A standard that's certified for that type of extraction. So if you see change, it might be the change due to the laboratory doing the analysis as opposed to change that's occurred in the environment.

Irene Anderson: When did you plan to do this study? Next spring, June, July?

Eric Crecelius: We didn't set a schedule. The sampling would be done in the middle of a summer. That's to avoid the problems of ice and storms. There was some interest in having parts of the sediment work occur early in the season, middle of the season, and late in the season to look at a few selected sites, to look at change in sediment chemistry over time, to see if there's some trend that occurs during the summer season.

One option would be to have a main oceanographic cruise sometime in the middle of the summer. This oceanographic cruise, I'm assuming, would incorporate other disciplines, biological sampling and maybe other types of work. I don't know if there would be any physical oceanography done at the same time.

But in addition, it might be possible to go out to some of these sites with a small boat, even when there's ice, or on a snowmobile, and collect surface sediments with a relatively lightweight sediment sampler to look at some temporal trends in surface sediment at some of the sites. But the main effort would be during the middle of the summer, to look at the water column and the surface sediments at a number of sites. It would be desirable to repeat this 2 or 3 years in a row before dredging begins.

Charlotte MacCay: You reference the state-of-the-art sampling vessel. Were you envisioning something different than you illustrated in your slides in the first day presentation?

Eric Crecelius: Right. What I showed in the slides is the minimum research vessel, which was an old gill netter.

Charlotte MacCay: Did you refer to some procedure you needed to do within 8 hours? Is it your assumption that that would be done on the vessel?

Eric Crecelius: Right.

Charlotte MacCay: So that you would eliminate any run time into Nome, or to any other laboratory?

Eric Crecelius: Right. There may be access to an oceanographic vessel, like the *Alpha Helix*, that would be available from the University of Alaska, or a NOAA research vessel. These are vessels that are 100 to 200 feet in length, and cost $10,000 or $20,000 a day. With one of those vessels, you could do everything on shipboard.

The fallback position is you use a fishing boat, a small boat, to collect the samples, run back to shore maybe to do filtering or, squeezing, in a laboratory facility that might be available. Either a temporary makeshift one, or – sometimes John said he's done it a the high school chemistry lab.

Charlotte MacCay: Well, that raises my second point. Sometimes when the baseline studies are designed to be cost effective, then you run into logistical problems or sampling device failure, and end up with so few samples that you can't then compare your baseline data to your monitoring data. I don't know that you've addressed that, but you did reference a relative bare bones program. I'm wondering if you considered, or assumed, that you might lose a certain number of samples, and what that would do to the statistical validity of the program?

John Trefry: This could all be done from the *Eager Beaver*, the small boat. So everything that's on this list could be done from a minimal vessel. I don't think there's anything on here that couldn't be done under those conditions.

Eric Crecelius: It could be done, but the disadvantage would be that, you wouldn't have accurate navigation. If you had an oceanographic vessel, you could make a profile with a CTD. You could use a built-in current meter to obtain additional data. Plus all the work could be done on shipboard, the filtering and pore water processing. You would have a higher rate of success as far as completing everything on time, and high quality.

But if no one wants to pay half a million dollars for the research vessel, and you still want to have a monitoring program, then you could charter a boat for $300 a day and get essentially the same quality of data. But it would be a little riskier as far as completing everything.

Charlotte MacCay: Right. My comment is just that when the program is designed, or the particular proposal selected, that those things be considered. If it has to be done within 8 hours, presumably you're going to do all three of your stations at once before you run in to do that. And if you've had any down time or sampling device failure, what tends to happen is that a certain number of the samples drop out and can't be used. And what you end up with is baseline data that is not easily comparable to the actual monitoring data. It's something to consider in the program design.

Eric Crecelius: Yes, that's a good point.

CHAPTER 3

FOOD CHAIN AND BIOACCUMULATION SESSION

Ronald Eisler, Ph.D., Session Chair
Anton Scheuhammer, Ph.D., Assistant Session Chair

POTENTIAL HAZARDS OF SEDIMENT INTERSTITIAL WATERS TO MARINE BENTHIC ORGANISMS

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Sediment interstitial waters usually contain elevated levels of metals when compared to the overlying water column. Extensive dredging may release metals and other compounds from interstitial waters into the surrounding ecosphere with significant biological effects.

In one case study, an abandoned electroplating plant that had discharged waste continuously for 30 years into Narragansett Bay, Rhode Island, had measurable effects on metals contents of sediments, biota, and interstitial waters. Moreover, the area had been extensively and periodically dredged in order to enlarge existing ship channels. Field studies showed elevated levels of silver, cadmium, cobalt, chromium, copper, iron, manganese, nickel, lead, and zinc in surficial sediments and benthic organisms near the discharge outfall, an impoverished molluscan faunal community near the outfall, elevated moisture content in clams nearest the plant, and sediment interstitial water concentrations that were 13X to 50X higher than the overlying water column for Cd, Cu, Ni, and Pb, 333X higher for Zn, and 818X higher for Mn.

Laboratory studies were conducted in flowing raw seawater with softshell clams (*Mya arenaria*) in order to measure the effects of simulated sediment interstitial water solutions on bioaccumulation and survival. Final calculated concentrations of the toxicant solution were 7,200 μg/ℓ manganese, 2,500 μg/ℓ zinc, 70 μg/ℓ lead, 50 μg/ℓ nickel, 50 μg/ℓ copper, and 1 μg/ℓ cadmium; these concentrations approximated the highest measured levels for these metals within surficial interstitial sediment waters from mid-Narragansett Bay, Rhode Island. *M. arenaria* were also subjected to a 20% solution, i.e., 1,440 μg/ℓ Mn, 500 μg/ℓ Zn, 14 μg/ℓ Pb, 10 μg/ℓ Ni, 10 μg/ℓ Cu, and 0.2 μg/ℓ Cd. One study was conducted for 112 days in winter at 0 to 10°C, and another for 16 days in summer at 16 to 22°C. In the winter study, all clams exposed to a 100% solution died between the 4th and 10th week; soft parts of survivors at 6 weeks contained about 19 times more Pb, 15X more Zn, 12X more Cu, 10X more Mn, 3X more Ni and 0.1X more Cd than controls; relatively minor changes in whole body elemental content of calcium, chromium, iron, potassium, magnesium, sodium, strontium and vanadium were observed. Clams exposed to a 20% solution during winter survived the 112 day study; at that time these contained about 5X more Cu, 4X more Mn, 3X more Zn and about 2X more Pb than controls; comparatively minor changes were observed in other elements examined. In the summer study, all *M. arenaria* subjected to the 100% solution died between 6 and 14 days; survivors from this group at 7 days contained about 25X more Pb, 13X more Cu, 11X more Zn, 7X more Mn, and 3X more Ni than controls; other changes in elemental content were not as pronounced. Mortality in the 20% group during summer was slightly higher than controls during the 16 day study; at 14 days survivors from this group contained about 12X more Mn, 7X more Pb, 7X more Zn, 4X more Cu, and 3X more Ni than controls. Survival and bioaccumulation patterns were not altered through feeding a supplemental diet of algae. Under the constraints of the study, the probable cause of reduced survival of clams was due to copper, although zinc at the levels tested may have had some influence.

The implications of these findings to the proposed dredging operations in Norton Sound are discussed.

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PRESENTATION AND DISCUSSION

Based on the most recent Draft Addendum on proposed mining activities in Norton Sound, three points are seen notable: (1) The barge effluent contains 22 ppb of mercury measured at the edge of the mixing zone, which assumed a 35 fold dilution factor; (2) each barge used 181 million liters of seawater daily; and (3) the proposed mining season was set at 100 days annually. Computations suggest that each barge discharges 0.3 lb of mercury daily, about 31 lbs annually, or 430 lbs. (195 kg) over the length of the 14 year lease. Because each barge contains about 2035 kg of liquid mercury, an accidental sinking would introduce much greater quantities of mercury into the environment[1].

But I am not going to talk about mercury today. Instead, I will speak on dredging effects of marine ecosystems. Norton Sound dredging proposals suggest that each barge will dredge 100 surface acres over a 100 day season, to a depth of 3.6 m, and remove 1.5 million m^3 of sediments annually. This is equivalent to 5 football fields dredged daily per barge to a depth of 12 ft, or 500 football fields yearly to a depth of 12 ft over a period of 14 years.

I became concerned about dredging effects on marine benthos about 15 years ago, while studying Rhode Island electroplating plants. We now know that when bottom sediments are retrieved, the water that drains freely is not interstitial water; interstitial waters are usually bound to anoxic sediments. Interstitial waters are collected from sediments under anoxic conditions,

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[1] (Editor Note: The above scenario was based on unreliable data. No liquid mercury will be aboard any operating barge.)

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and removed under pressure in an oxygen free environment. Once oxidized, interstitial waters are quite soluble and measurable. Sediment interstitial waters usually contain elevated levels of potential contaminants when compared to the overlying water column. Dredging, storms, or other environmental perturbations accelerate the release of interstitial waters into the surrounding ecosphere with the potential to cause harm to marine benthic communities, including reduced survival, high bioaccumulation, and changes in species diversity. These effects were most pronounced at elevated water temperatures, and benthic non-mobile species seem to be the group at greatest risk.

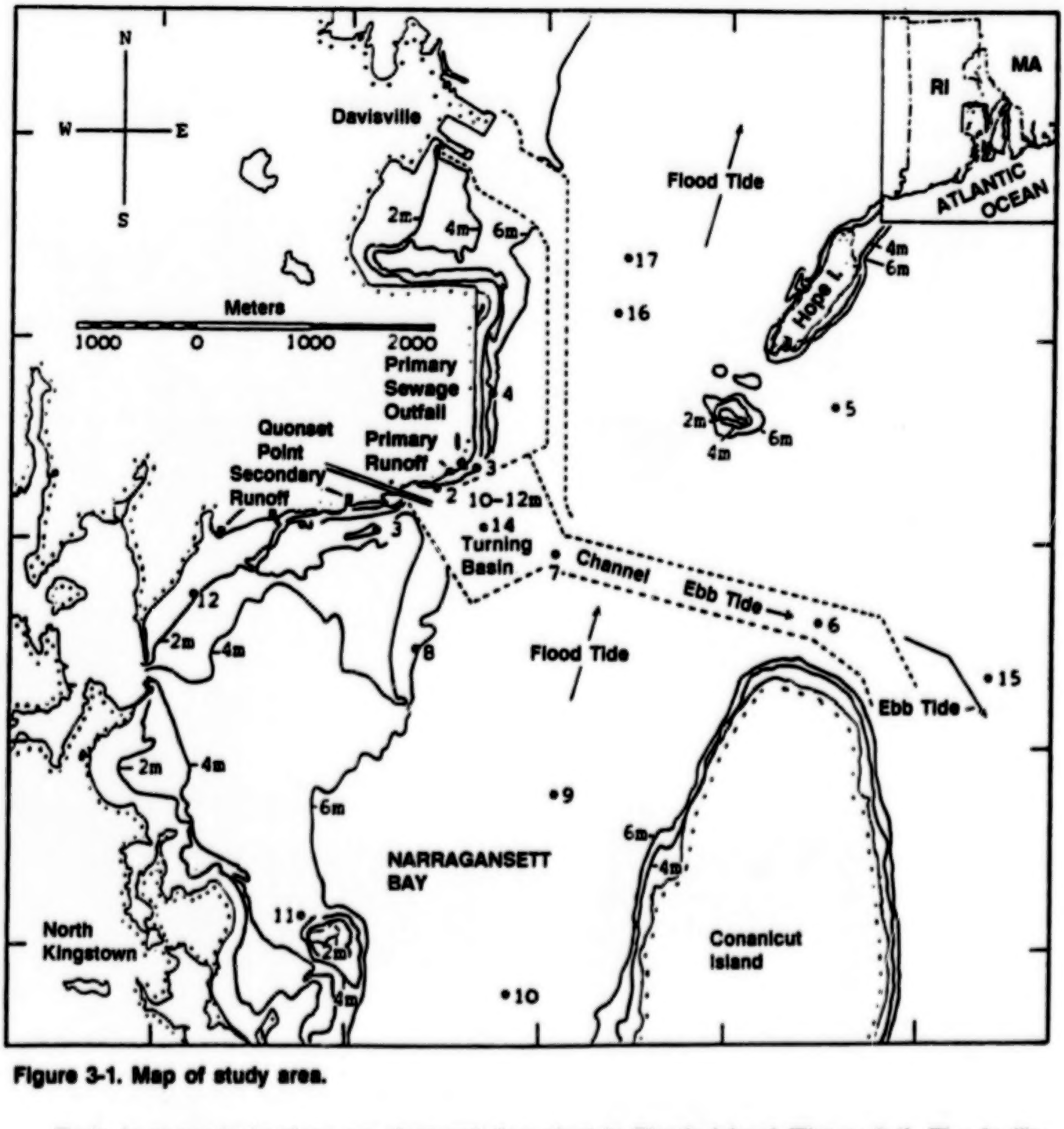


Figure 3-1. Map of study area.

Today's scenario involves an electroplating plant in Rhode Island (Figure 3-1). The facility was a Naval Air Re-Work Facility (NARF), essentially a garage for aircraft. Scheduled aircraft maintenance is conducted after several hundred hours of flying time. Naval aircraft were serviced at one of eight NARFs throughout the country. There, its engine was dismantled, acid stripped, and replated. In the process, large – but unrecorded – quantities of metals, phenols, cyanides, grease, and oil were discharged into Narragansett Bay, Rhode Island. The Quonset Point, Rhode Island, NARF operated between 1940 and 1970, when the facility closed. In 1973, a study was conducted by EPA, over a one-year period to measure potential impacts, if any, that this particular point source had on the Narragansett Bay ecosystem.

Narragansett Bay is very shallow, average depth about 10 m (30 ft). Periodically, the area within the dotted line (Figure 3-1) was dredged to enlarge the existing ship channel. Further to the north, near Providence, RI, a great number of electroplating plants are located – possibly the greatest density of electroplating plants in the world. The sediments in some areas of Upper Narragansett Bay are so enriched in metals that reclamation may be economically feasible. In this study, 17 stations were sampled (Figure 3-1) and a spectrum of metals was measured in the sediments in the overlying water column, in the molluscan community, and in sediment interstitial waters. Sediment cores to a depth of 2 m were taken and analyzed. Results of this study have been published (see References). During the course of a year, it was found that the 17 stations contained a total of 14 different species of molluscs, but that no station ever had more than 8 species (Figure 3-2). Stations that were nearest the primary sewage outfall and in the turning basin and ship channel usually had only one or two species of molluscs. One species of mollusc was present at more stations than others: the widgeon clam, *Pitar morrhuana*. *Pitar* were collected from 12 of the 17 stations (Figure 3-3). It is noteworthy that at the three stations closest to the sewage outfall, no *Pitar* were collected despite repeated sampling. The wet to dry ratio of *Pitar* soft parts was higher nearer the outfalls, suggesting that the organism is under stress. Organisms that accumulate contaminants, in general, tend to have elevated wet/dry ratios in selected tissues.

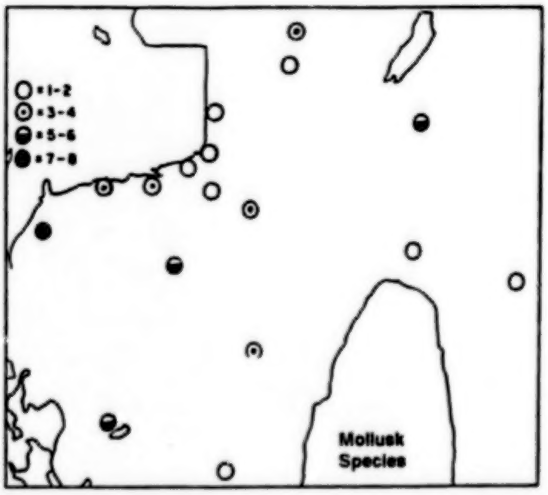


Figure 3-2. Number of species of mollusks collected at individual stations (total number of Mollusca species collected from all stations was 14).

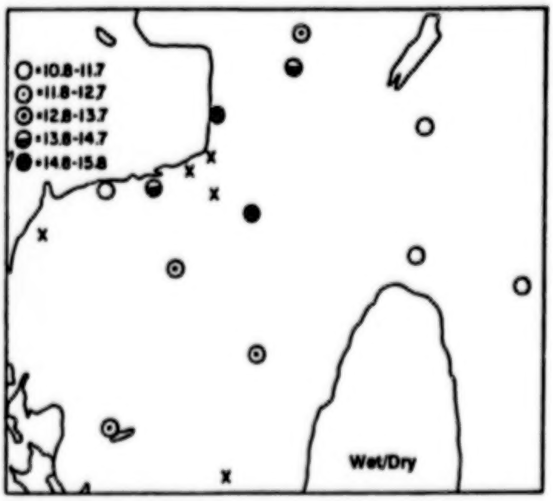


Figure 3-3. ***Pitar morrhuana*****.** Mean wet:dry ratios of clams collected from individual station. No *P. morrhuana* were collected at stations marked with an X.

Elevated levels of silver, cadmium, cobalt, chromium, copper, iron, manganese, nickel, lead, and zinc, were found in clams nearest the

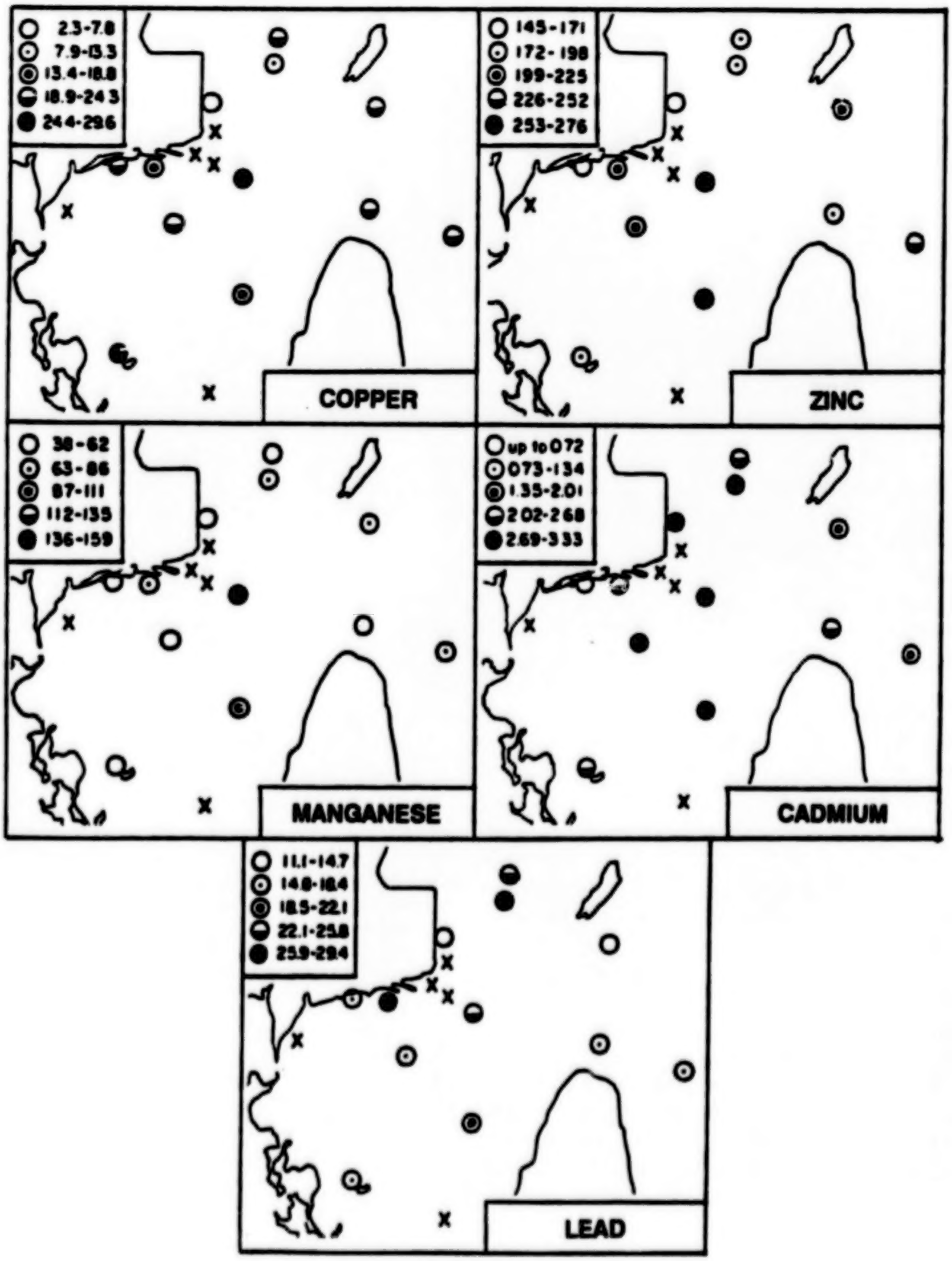


Figure 3-4. ***Pitar morrhuana*****. Concentrations of five metals in widgeon clams from 17 stations in NARF study. Values are in mg/kg dry weight soft parts.**

50

outfall, based on dry weights. Values in *Pitar* for copper, zinc, manganese, cadmium, and lead are shown in Figure 3-4.

In terms of maximum concentrations measured in the study area, a number of observations can be made (Table 3-1). Metal levels in sediment interstitial waters, when compared to that of the water column, were significantly higher. For example, cadmium, copper, nickel, and lead values in the sediment interstitial waters are 13 to 15 times higher than the overlying water column, about 300 times higher for zinc, and 820 times higher for manganese. Metal values in clams, when compared to sediments, were about the same for cadmium and nickel. But clams had 2/3 less copper than the sediments, 1/2 the manganese, 2/3 the lead, and 64% more zinc. There was no statistically significant correlation for any metal for clam vs. sediments, or sediments vs. water column. From the field portion of this study it was evident that sediment interstitial waters contained much higher levels of metals than the overlying water column.

Table 3-1. Maximum concentrations measured in study area.

	Sediments mg/kg dry wt.	Water Column µg/ℓ	Sediment Interstitial Waters µg/ℓ
Cadmium	2.5	0.08	1
Copper	98	1.0	50
Manganese	307	9	7200
Nickel	34	1.5	50
Lead	81	<1.0	70
Zinc	168	8	2500

In laboratory studies, the hypothesis was tested that a cocktail simulating sediment interstitial water could account for the elevated metal residues found in the clams and the impoverished molluscan community.

A metals mixture was prepared that delivered 7200 ppb of manganese, 2500 ppb of zinc, 70 ppb of lead, 50 ppb of copper, 50 ppb of nickel, and 1 ppb of cadmium, and represented the maximum amount of individual metals released to the water column from NARF sediments if metals became immediately available. Laboratory studies were conducted in raw, flowing sea water. Uncontaminated seawater contained between 0.06 ppb (cadmium) and 10 ppb (zinc). In seawater particulates, this range was < 0.01 ppb for copper to 0.8 ppb for manganese (Table 3-2). The metals mixture was added to experimental 24 ft troughs and took about 5 minutes to transverse this distance. The addition of 7200 ppb manganese in water and particulates was measured after passage. Very little Mn was in particulate material, about 4 ppb (Table 3-2). Similarly with Zn, 2500 ppb were added and 2460 ppb measured; 5 ppb of Zn were in the particulates. With Pb, 70 ppb, was added and 56 ppb measured; 5 ppb of Pb was in the particulates. With Ni, 50 ppb were added and 59 ppb measured; 1 ppb Ni was in the particulates. With copper, 50 ppb, were added,

Table 3-2. Dissolved and particulate concentrations of six metals in seawater after passage through troughs containing softshell clams.

	Mn	Zn	Pb	Ni	Cu	Cd
CONTROLS						
Dissolved	8	10	<1	2	1	0.06
Particulate	0.8	0.04	<1	0.3	0.01	<0.06
EXPERIMENTAL[a]						
Dissolved	7200	2460	56	59	37	1.3
Particulate	4	5	5	1	2	<0.06

[a] Control levels plus calculated addition, in µg/ℓ, of 7200 Mn, 2500 Zn, 70 Pb, 50 Ni, and 1 Cd.

37 ppb measured in the water column, and 2 ppb in particulates. With cadmium, 1 ppb was added and 1.3 ppb measured. There was very little Cd in the particulates. The conclusion is that after approximately five minutes in the water column, metals remained in the water column and were not adsorbed onto detritus or foreign materials normally found in raw sea water.

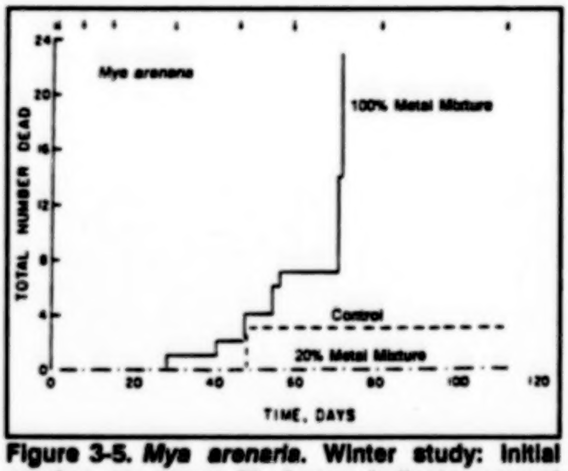


Figure 3-5. *Mya arenaria*. Winter study: Initial number per group 55. Arrows indicate removal of five survivors. Temperature range 0°C at start to about 10°C at day 112.

Using softshell clams, *Mya arenaria*, groups were dosed at 100% or 20% pore water, under two dietary regimes (no effect) and two thermal regimes. One study was conducted in the winter and lasted 112 days. The water temperature ranged from about 0° at the start, and gradually increased to about 10°C. Survival was excellent during the first six weeks (Figure 3-5). However at seven weeks, clams exposed to the 100% metal cocktail started to die and by the end of eight or nine weeks, all had died. The "arrows" at the top of Figure 3-5 represent when some specimens were removed for metals analysis. There was no difference in survival between controls and the 20% metal mixture in the winter study.

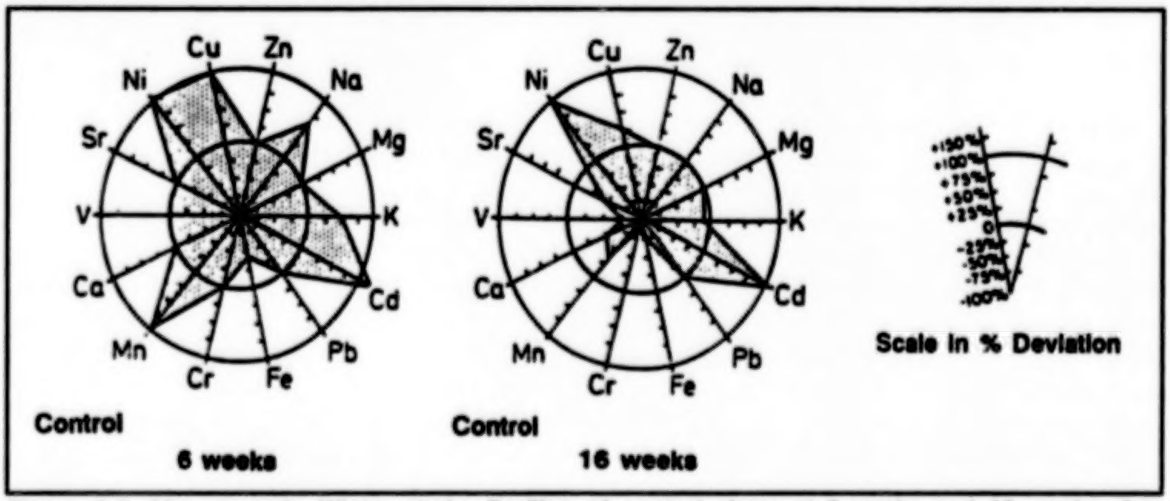


Figure 3-6. *Mya arenaria*. Winter study: Profiles of control clams at 6 weeks and 16 weeks. Any deviation from zero is statistically significant at the 0.05 probability level. (Control values at start, in mg/kg dry weight, were: Cd, 0.3; Ni, 2.1; Cr, 4.4; Pb, 6.6; V, 8.4; Cu, 9.8; Mn, 29; Sr, 63; Zn, 93; Fe, 4834; Ca, 10,522; Mg, 11,609; K, 13,936; and Na, 73,970.

The elemental composition of control softshell clams (Figure 3-6) suggest that clams can regulate their elemental composition over time. The profile technique in Figure 3-6 is essentially a series of bar graphs arranged radially. Values for individual metals at the start were set at 100%. Any change from the zero circle is statistically significant. At six weeks, changes were recorded in cadmium, sodium, copper, nickel, and manganese - and these are control clams. At 16 weeks, a different pattern was evident: elevated levels of nickel and cadmium, and depressed levels of iron, chromium, manganese, calcium, vanadium and strontium. I am not going to discuss any elements except those added in the cocktail, because there was essentially very little change in these additional elements. Because of measured osmoregulatory changes in control clams, the experimentals were compared to controls at the same moment in time, in this case, 6 weeks and 16 weeks.

Profiles of clams exposed for 16 weeks to the 20% metal mixture (Figure 3-7) show significantly elevated levels of copper, zinc, lead, and manganese; this pattern was not as clearly defined for that group at six weeks. Profiles of clams exposed to the 100% metal mixture at six weeks show elevated levels of zinc, copper, nickel, manganese, and lead (Figure 3-7). All clams exposed to the 100% metal mixture were dead by 16 weeks under winter conditions.

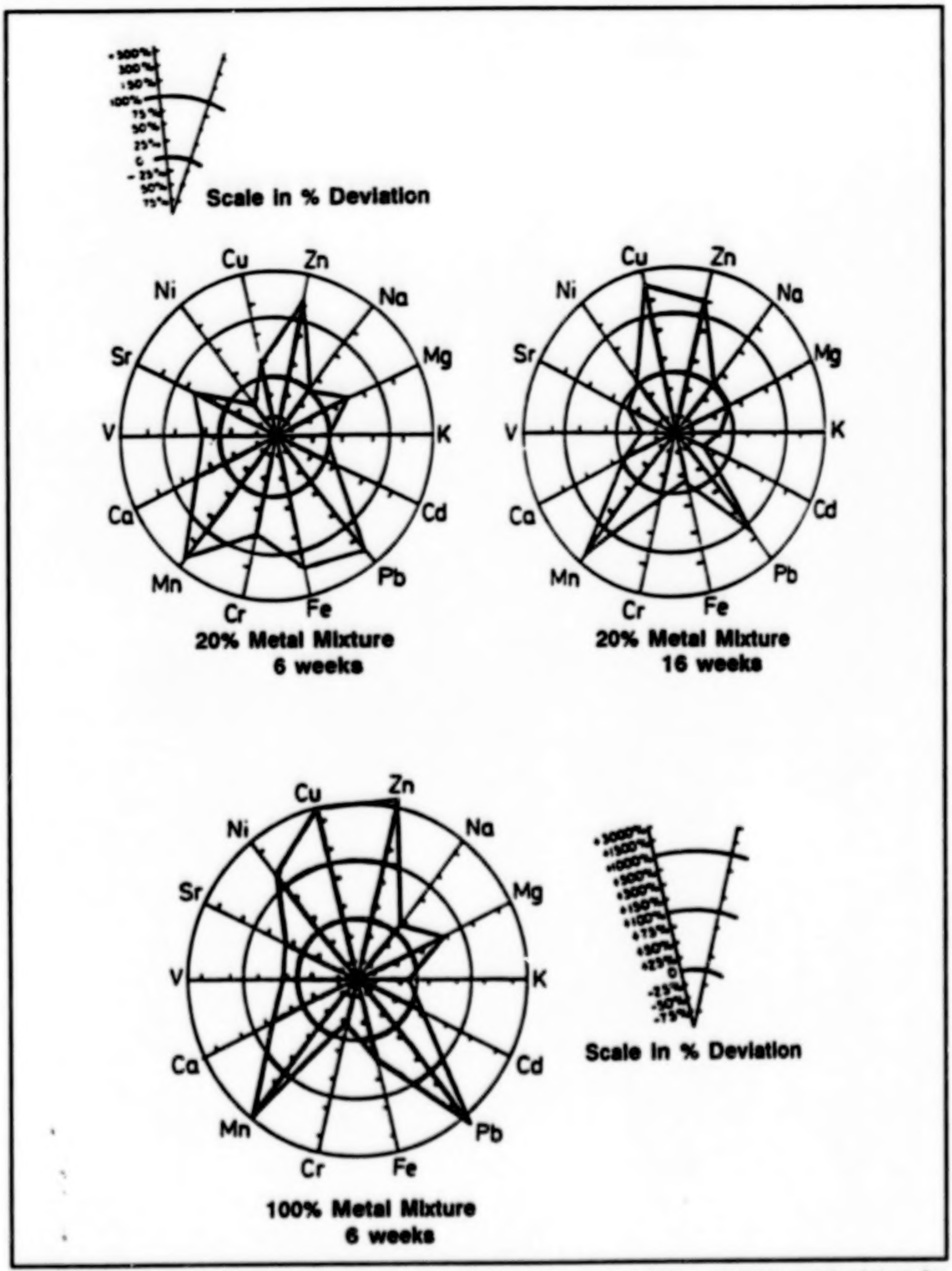
This study was repeated under summer conditions, where the water temperature ranged between 16° and 22°C (Figure 3-8). All clams exposed to the 100% metal mixture group were dead in only 16 days, regardless of dietary regimen. The 20% metal mixture group had elevated mortality; a profile of this group after 24 hrs (Figure 3-9) suggests that manganese is the first element taken up initially. Clams in the 100% group exposed for 24 hours under summer conditions show profiles similar to those exposed to a 20% mixture for four months under winter conditions. It is concluded that temperatures significantly affect accumulation rates, but not patterns.

After 14 days (Figure 3-10), all clams exposed to the highest cocktail were dead. Those exposed to the 20% mixture under summer conditions had a profile similar to those exposed to a full strength cocktail for four months under winter conditions, again demonstrating the impact of water temperature on accumulation rates.

The cause of mortality in *Mya* exposed to interstitial waters was determined by bioassays conducted throughout the year at different water temperatures (Table 3-3). At the very highest temperature and seven days exposure, LC-50 values were 150 ppb for cadmium (max. concentration tested was 1 ppb); 8800 ppb for lead (tested 70 ppb); 300,000 ppb for manganese (tested 7200 ppb); and more than 50,000 ppb for nickel. In effect, Cd, Pb, Mn, and Ni probably did not contribute to or cause any mortality. In the case of copper, there were no deaths in two weeks at 3000 ppb at 4°C, 50% dead in two weeks at 86 ppb and 17°C, and 50% dead in seven days at 35 ppb and 22°C – suggesting that Cu could account for most, if not all, of the mortality in the high exposure dose. Zinc also showed the same pattern, and under summer conditions could probably contribute to the mortality of the clams. The possibility also exists that zinc and copper may act synergistically, as in the case with fish.

What are the implications of these findings, if any, to the proposed Norton Sound Mining Program? Because the sediment interstitial waters (the waters surrounding and bound to the bottom sediments) contain elevated levels – at least of metals – when compared to the overlying water column, then dredging would probably release the sediment interstitial waters into the surrounding ecosphere with the potential to cause harm to marine benthic communities. These adverse effects include reduced survival, elevated metal accumulations, and changes in species diversity. These effects would be exacerbated at higher temperatures.

In terms of baseline monitoring of Norton Sound, several approaches should be considered. First, chemical analysis of the sediment interstitial waters to a depth of 3.6 m as well as the overlying water column, sediments, and representative sentinel organisms should be determined.



20% Metal Mixture
6 weeks

20% Metal Mixture
16 weeks

100% Metal Mixture
6 weeks

Figure 3-7. *Mya arenaria*. Winter study: profiles of clams exposed to 20% and 100% metal mixture for 6 or 16 weeks. Values are in significant percent deviation from controls of identical time-diet regimen; any deviation from zero is statistically significant at the 0.05 probability level or lower.

The selection of the chemicals would be based on historical waste disposal and mining practices. Suitable indicators could include filter-feeding bivalve molluscs. These measurements should extend over a 12-month-period at least. After these data are analyzed, predictions on ecotoxicological effects of sediment interstitial waters released by dredging would be possible using published toxicity data, quantitative structure activity relationships, and other data bases. The hypothesis can be confirmed or modified in laboratory studies using suitable test organisms. During the mining operation, changes in species diversity can be monitored with dredge samples, and bioaccumulation rates of caged samples at different distances, times, and depths.

Don Hansen: Could you repeat the depths that you said that you would be sampling for interstitial waters?

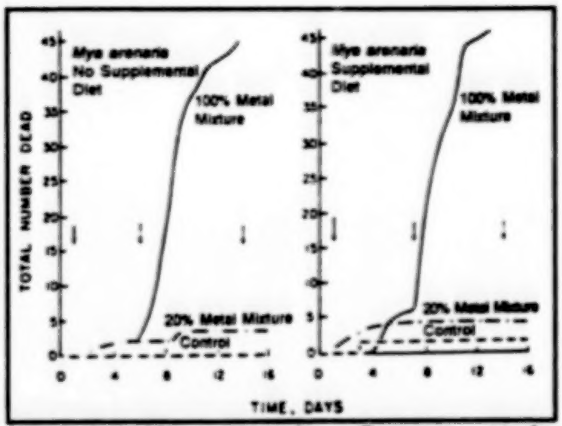
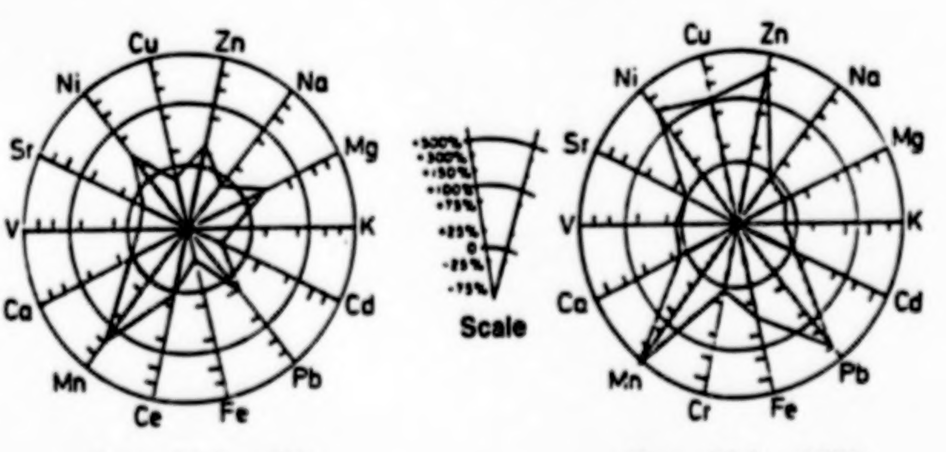


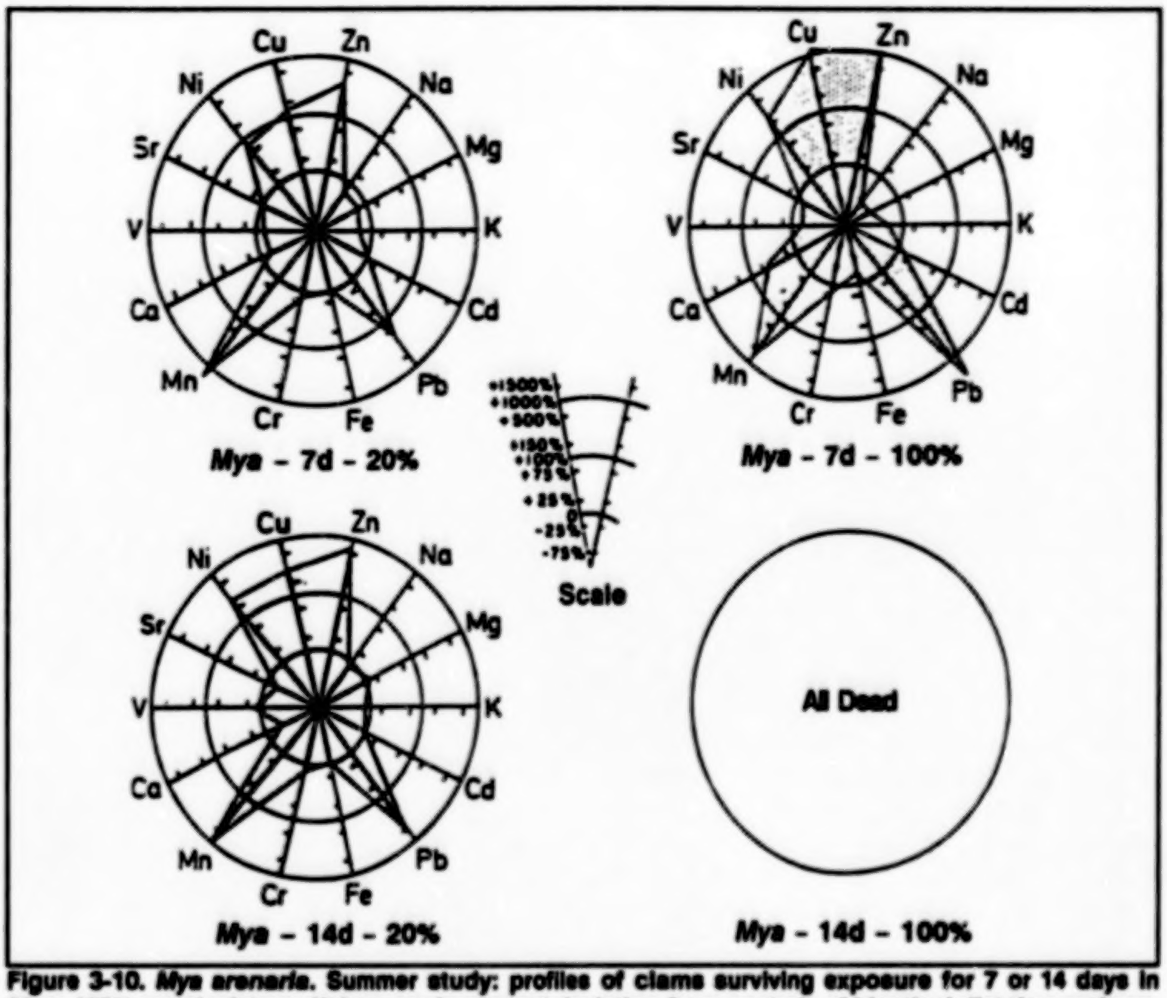
Figure 3-8. *Mya arenaria*. Summer study: mortality among clams held in flowing raw seawater during exposure to 20 or 100% concentrations of a mixture of metal salts. Arrows indicate intervals when 5 survivors were removed from each group; initial number per groups was 55.



***Mya* - 24 h - 20%**

***Mya* - 24 h - 100%**

Figure 3-9. *Mya arenaria*. Summer study: profiles of clams surviving immersion for 24 hrs in 20 or 100% metal mixture. Values are in percent deviation from controls of identical diet-time regimen.



Mya - 7d - 20%

Mya - 7d - 100%

Mya - 14d - 20%

Mya - 14d - 100%

Figure 3-10. *Mya arenaria*. Summer study: profiles of clams surviving exposure for 7 or 14 days in 20 or 100% metal mixture. Values are in percent deviation from controls of identical diet-time regimen.

Ronald Eisler: Down to 3.6 m. The proposed dredging depth is 3.6 m, and that is very possibly where anoxic sediments are located. There are few metals bound to the highly oxygenated surface sediments.

Tony Giordano: Would you quickly go over the arsenic and the bioaccumulation that we were talking about?

Ronald Eisler: A book called *Arsenic Hazards to Fish, Wildlife and Invertebrates, a Synoptic Review* was published by the USFWS in January 1988. In general, arsenic hazards to man from the marine environment, in my opinion, are overrated. Most of the arsenic in marine organisms is in the form of arsenobetaine. There are some 2-year studies, using rats wherein enormous quantities of arsenobetaine were fed to rats with no measurable adverse effect. Inorganic, trivalent arsenic is quite toxic, but it oxidizes very quickly in the marine environment to pentavalent and organic. I don't think it is a problem, unless a point source is involved.

Table 3-3. Acute toxicity of various metals to adult softshell clams.

Copper	
4°C	LC-50 (336 hr) <3.00 mg/ℓ
17°C	LC-50 (336 hr) = 86 µg/ℓ
33°C	LC-50 (168 hr) = 35 µg/ℓ
Zinc	
4°C	LC-50 (336 hr) <25.0 mg/ℓ
18°C	LC-50 (336 hr) = 2650 µg/ℓ
22°C	LC-50 (168 hr) = 1550 µg/ℓ

At 22°C and 168 hr, LC-50 (168 hr) values, in µg/ℓ, were 150 for Cd, 8800 for Pb, 3000,000 for Mn, and more than 50,000 for Ni.

Eric Crecelius: There are also studies that have been done on chimpanzees and dogs where they have fed them large amounts of arsenic-rich seafood. They couldn't observe any effects on them and they didn't observe much of any bioaccumulation either. Those were Canadian studies.

Ronald Eisler: Actually the only species that is shown to be sensitive over time to arsenic is man. It takes about 50 years for the syndrome to develop in man. It has never been repeated in any animal model as far as I know, possibly because of the lengthy exposure. In certain areas where the drinking water is high in arsenic, and where the people seldom leave, the effects are potentially serious. But they are reversible when the arsenic is removed.

Ray Emerson: Do you think that your copper question could be related to something like absorption kinetics? In other words, if you've got your cocktail with a variety of divalent cations there and one is going to get to that action site, maybe before the other, is that a possibility?

Ronald Eisler: You asked that question before and I thought it was an excellent question then as I do now. The results of the study, show that 95% of the copper was in the water column, and 5% was bound to sediments. Molluscs start feeding when they are triggered by particulate materials. Somehow they have an internal mechanism to determine if there is any food value in filtered particulates. If there is no food value, it usually is excreted in the feces. Otherwise, it gets incorporated. I can't answer that question as to whether molluscs can break the Cu bond. But I would suspect that in oxygenated water, most of that sediment interstitial water would not be bound to the particle. That is what we are asking. Once it is oxidized it is released and available.

Anton Scheuhammer: What part would the marine invertebrates play in a monitoring or baseline study?

Ronald Eisler: Benthos form an integral part of the food chain. Molluscs are important. In this area, molluscs are the basis of the food chain for walrus, and probably certain whales. Bivalve molluscs filter the water, and are usually non-mobile. In my opinion, non-mobile species represent those at greatest risk to dredging activities. Echinoderms are another possible group of indicator organisms. But I don't know if there is a database on the subject, or on echinoderms specifically. Polychaete annelids have been used, but in general, insufficient numbers were collected for isopleths, as was the case with widgeon clams. There are also problems with taxonomic identification of polychaetes. The NARF study area, incidentally, consisted of mucky, organic silt, clay, 40% TOC. In fact most of Narragansett Bay is closed to the taking of clams, not as a result of the electroplating wastes, but from human sewage discharged into Narragansett Bay.

_____: In terms of your statement about arsenic not being very hazardous, could you comment? I don't want to take the EPA Criteria as Bible either, but EPA has a human health criteria which sets a water concentration of arsenic, for when you are going to eat marine foods, at 17.5 ng/l or 17.5 parts per trillion. They equate that to a one in a million chance of getting cancer from eating marine seafood.

Ronald Eisler: I have often wondered about that criterion, and have had problems when it was being formulated. Why should something that would protect 95% of the species be allowed if in some cases the other 5% of the species would contribute more than half of the biomass. I have reservations on how the cancer increase potential was calculated, as well as with the basic Ames Test. I endorse growth, survival, reproduction, behavior, and metabolism as study variables as well as bioaccumulation. Risk assessment is at best a speculative situation. It is uncertain, yet people are forced to produce quantitative numbers. In the case of polychlorinated biphenols, those criteria that are designed to protect humans may very well do so, but it will completely eliminate the mink ranch industry. Two ppm in our diet is harmless, but the same amount in mink diets causes reproductive inhibition. Mink receive PCBs from Great Lakes fish. The PCB levels permitted in fish is expected to be modified downwards in time. Risk assessment, as currently practiced is one part biological or toxicological, and another part economic, social and political. The last part accounts for most of it. But biologists would be remiss if they did not respond with informed decisions based on accurate information.

ACCUMULATION AND TOXICITY OF MERCURY, CADMIUM, AND LEAD IN VERTEBRATES

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Ron Eisler and I agreed today that he would speak about the lower trophic levels of food chains and that I would talk primarily about mammals and birds. The three metals that I am going to focus on today are those normally considered to be the most toxic metals to the higher trophic levels, and they are mercury, cadmium and lead. I'm afraid that I have very little expertise regarding the toxicology of some of the other metals that may be of importance toxicologically to lower trophic levels, including copper and zinc.

I work with the Canadian Wildlife Service and what we are concerned about in terms of wildlife toxicology are three basic issues. First, we are concerned about the wildlife species themselves. Are there any risks to their health or reproductive success? Secondly, we are concerned about levels of contaminants in the various organs and flesh of these animals because they are sometimes consumed by humans. We are concerned about exposure to humans through the food chain by eating these wild foods. Thirdly, we are interested from the point of view of overall environmental quality. We oftentimes use various wildlife species as indicators of increased or decreased bioavailability of various contaminants.

MERCURY

The first metal I will talk about is mercury. Now, when I say mercury I mean primarily methylmercury and some other forms of organic mercury. It is methylmercury that is particularly important toxicologically because methylmercury is absorbed from food very efficiently. Almost 100% of the ingested dose is absorbed and this is very much more than inorganic forms of mercury. Inorganic forms of mercury are nevertheless important in food chain bioaccumulation because, in principle, any form of mercury can be methylated.

There are some well-documented relationships concerning the behavior of Hg within food chains. We know that carnivores accumulate more mercury than other animals and we know that this is especially true in aquatic food chains. We also know that fish eaters or other animals that prey on fish-eaters accumulate the highest levels of mercury. These are the animals that we consider to be at the greatest risk in response to an increased environmental availability of mercury.

The major toxic effect of methylmercury - if the dose is high enough - is death. If the dose is not high enough to cause death, there may nonetheless be neurological impairment. This takes the form of decreased ability to coordinate movement and the visual system is affected. Certainly any animal that is a visual predator is very much at risk for this type of neurological impairment because they depend on their visual system to catch food. Lastly, even if the level of exposure is not high enough to cause neurological impairment, there may be reproductive impairment. In the case of birds, there is a decreased hatchability of eggs, increased mortality of hatchlings; and in mammals, increased rate of death of newborns, and malformations. These effects can occur at levels that are approximately one fifth of those that are required to produce neurological impairment in adults. Reproductive effects can also occur in humans, as we will hear from Dr. Marsh. Reproductive impairment constitutes the main focus of current research in methylmercury toxicology because these are the dysfunctions that occur at the lowest levels of exposure.

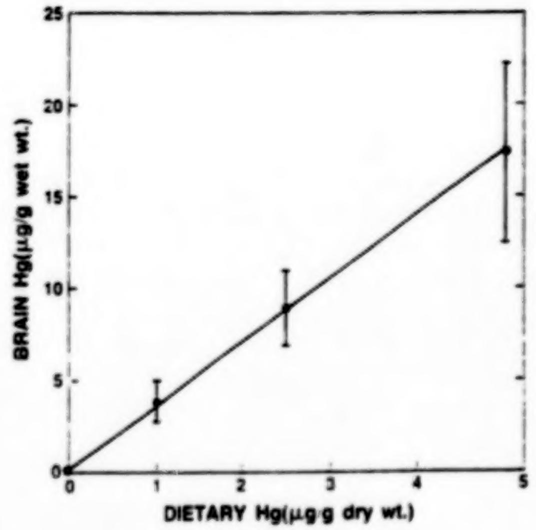

Figure 3-11. Accumulated (steady-state) level of mercury in brain tissue of zebra finches exposed to different dietary levels of mercury for 2.5 months.

Certain animals can accumulate high levels of mercury in their tissues, but because they have the ability to very efficiently demethylate mercury, the mercury is relatively nontoxic to them. A high proportion of the mercury that is present in the livers of these species is inorganic, not organic, mercury. Mercury and selenium tend to co-accumulate in livers of these same species. This has been shown very convincingly for certain marine predators, such as dolphins, seals, polar bears, and some other mammals associated with freshwater aquatic systems. It appears that these species of animals, which are normally exposed to fairly high levels of dietary methylmercury, have evolved a system of detoxification involving the demethylation of methylmercury. The resulting inorganic mercury associates with selenium and accumulates through time in some sort of mercury-selenium- protein complex that seems to be non-toxic to the organism itself. For example, adult polar bears can accumulate very high levels of inorganic mercury in their liver tissue over time due to demethylation of the naturally occurring methylmercury that they absorb from their food, and suffer no obvious signs of mercury intoxication.

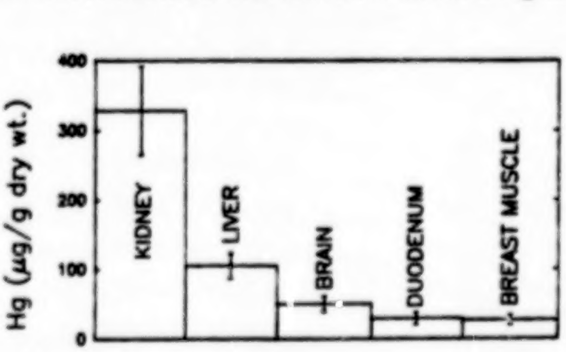

Figure 3-12. Accumulated levels of mercury in various tissues of ring doves exposed to a dietary level of 5 ppm mercury for 10 weeks.

The accumulation of mercury in tissues is closely correlated to the dose of mercury ingested. Figure 3-11 demonstrates that the dietary levels of mercury are very closely related to the steady state levels that accumulate in brain tissue. Mercury concentrations are approximately three to five times higher in the tissue than in the diet in the case of brain tissue from zebra finches exposed to dietary methylmercury. Figure 3-12 shows the typical distribution of mercury in various tissues from ring doves exposed to 5 ppm through the diet for 5-10 weeks. Kidney tissue tends to accumulate the highest levels, followed by liver, brain, and then breast muscle. Breast muscle accumulates about 20 to 25 ppm of mercury when the diet contains about 5 ppm. Almost no demethylation occurs in muscle tissue, as illustrated in Figure 3-13, which plots the percentage of the total mercury in the tissue that is organic. We estimate this percentage by doing two analyses, one for total tissue mercury, and a separate one for organic mercury. Most of the organic mercury is in fact methylmercury. In all three of the tissues in Figure 3-13, total mercury increased over the first 10 weeks of the experiment during which

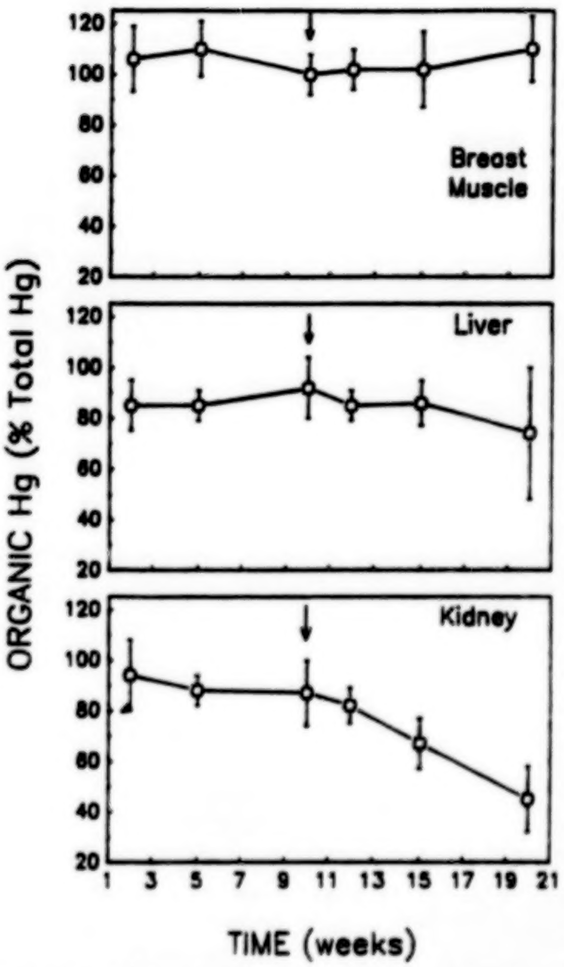

Figure 3-13. Percent methylmercury (organic mercury/total mercury) in ring doves during and after exposure to dietary mercury (at arrows), showing different demethytion rates in different tissues.

time methylmercury was present in the diet. Tissue mercury achieved a steady state at about 5 to 10 weeks of exposure. Then, after removal of mercury from the diet at 10 weeks, total tissue-mercury levels decreased and eventually returned to close to normal levels. But if we look at the percentage of total mercury that is organic, we see that, in breast muscle, it doesn't change at all. Virtually 100% of the mercury present in breast muscle is organic mercury, regardless of the duration of exposure. A similar result was obtained for liver tissue. These birds do not appear to react the way certain marine mammals do, in that they do not demonstrate much demethylation of mercury and storage in liver, at least not over the course of this experiment, which ran for 20 weeks. Thus, there is probably a degree of species specificity in the efficiency of the demethylation process. For doves, only in the kidney did the percent of methylmercury in the tissue drop from around 90 to 100% down to about 50% by the end of the study. There is a small amount of demethylation taking place, and inorganic mercury is accumulating in kidney, but not in the liver of these particular birds. The point to be made here is that an analysis of total mercury doesn't necessarily tell you what is toxicologically relevant, from the point of view of a consumer of these tissues. For consumers, it is the organic mercury content of the tissue that is particularly relevant.

Recently, I conducted a study to determine if there are changes in the selenium concentrations of tissues as a result of methylmercury exposure. In birds (ring doves), certain tissues, like breast muscle, lose selenium as their mercury levels increase, whereas other tissues, such as brain, tend to show an increased level of selenium as their mercury levels increase (Figure 3-14). In this study, the diet had a normal level of selenium (0.1 ppm) + 5 ppm mercury as methylmercury. The results indicate that organisms may redistribute their selenium stores, in response to methylmercury exposure. It is my belief that tissue selenium levels should be measured whenever mercury levels are measured. If one wants to make toxicologically relevant statements about a particular level of mercury in a tissue, then one should have an estimate of the total mercury concentration, the proportion of the total that is organic, and also the tissue-selenium level.

With regard to indicators of mercury exposure, I talked about this briefly last year (Scheuhammer, 1989a). For birds, I would suggest measuring mercury in eggs. Mercury deposition into the developing egg is very closely related to the average dietary methylmercury concentration. On the basis of controlled feeding studies that I have conducted, dry egg albumin tends to have approximately 5 to 6 times the mercury concentration that is found in the diet (Table 3-4). Increasing the dietary methylmercury concentration from 0.5 to 5 ppm caused an increase in the level of mercury in albumin from about 2.5 to 28 ppm.

Another appropriate indicator for methylmercury exposure in birds is feathers (Table 3-5). For mammals, of course, hair or fur would serve much the same function. There is a good correspondence between the level of mercury in the diet and the level of mercury that is deposited into the growing feathers. A further advantage of feathers, from an analytical point of view, is that the level of mercury deposited into the feather will be very much higher than the corresponding level in the food that the bird is eating. As an indicator of toxicity, anything above 15 to 20 ppm in feathers represents a significant risk of reproductive impairment.

CADMIUM

The next metal that I am going to talk about is cadmium. The main target organ for cadmium toxicity is the kidney. This is true for all mammalian and avian species. Cadmium has also been shown to affect bone. The toxic effects on bone are best illustrated by the outbreak of Itai-Itai disease in Japan, where many people were consuming cadmium contaminated rice and developed symptoms of decreased bone mineralization. This syndrome has not been observed, however, in other documented cases of chronic cadmium toxicity. It is probably a function of cadmium exposure combined with nutritional deficiencies, such as calcium deficiency. Most cases of cadmium toxicity in humans have been of industrial or other

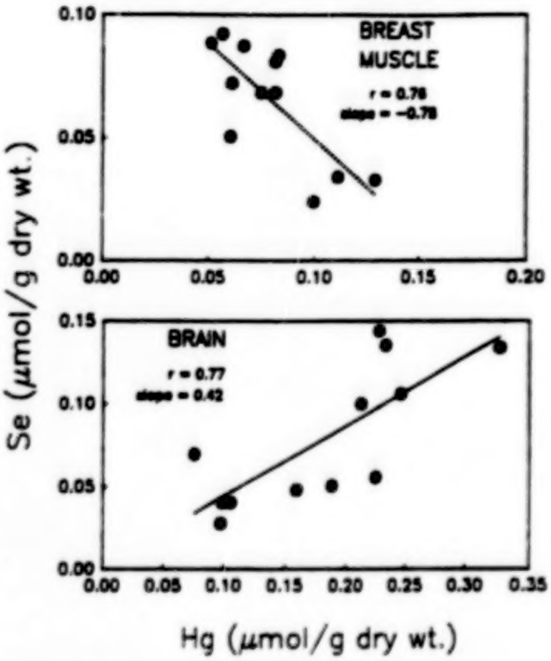

Figure 3-14. Loss of selenium by brain in response to mercury accumulation in ring doves.

Table 3-4. Bioconcentration factors in egg yolk and albumen at different dietary methylmercury concentrations (Hg concentrations in μg/g dry weight).

	Control	0.5 ppm Hg	5.0 ppm Hg
Albumen	<0.03	2.6 ± 1.1	28.0 ± 9.4
Yolk	<0.01	0.7 ± 0.4	6.8 ± 3.1

n=15/group

Table 3-5. Bioconcentration of mercury in the primary feathers of fledglings at different levels of dietary exposure (Hg concentration in μg/g dry wt.).

Diet	Fledgling Primary
0.02	0.4 ± 0.2
0.41	22 ± 4
4.64	212 ± 35

occupational origin where workers have accumulated critically high levels of cadmium in their kidneys.

The cadmium concentration accumulated in tissues is closely related to dietary levels, and to the duration of exposure. In ring doves, in which either a control diet or a diet containing 2 ppm, or 20 ppm of cadmium was consumed for up to six months, the concentrations that accumulated in liver tissue were approximately three to four times the levels contained in the diet (Table 3-6).

Table 3-6. Concentration of cadmium in the liver of ring doves exposed to different dietary levels for 6 months (adult) or 28 days (juveniles) (Cd concentration in µg/g dry wt.).

	Control Diet	2.0 ppm Cd	20 ppm Cd
Adult (Male)	1.3 ± 0.5	6.0 ± 1.3	49.2 ± 13.7
(Female)	1.5 ± 0.5	8.3 ± 2.7	63.5 ± 32.2
Juveniles	<0.3 (15)	0.36 ± 0.15 (16)	7.54 ± 3.09 (10)

Adults: n = 15/group

Cadmium is unlike mercury, in that it doesn't form naturally occurring organo-cadmium complexes, such as "methylcadmium." Inorganic cadmium is not absorbed from food nearly as efficiently as methylmercury. Cadmium is absorbed from the diet at probably less than 2% of the ingested dose. Nonetheless, once absorbed, it has a very long biological half-life, and it accumulates over time, particularly in kidney and liver. There is a very good correspondence between cadmium levels in target tissues (liver and kidney) and age. Seventy to 80% of the total body burden of cadmium is accounted for by the amount of cadmium that is in the kidneys plus the liver. Muscle and bone accumulate very little cadmium.

The wildlife species that are most at risk from increased cadmium availability are primarily herbivores because cadmium can be accumulated by certain plants to fairly high levels. For instance, the Ontario Ministry of Natural Resources has surveyed levels of cadmium in Ontario moose liver and kidney (Glooschenko *et al.* 1988). Kidney levels were much higher than liver, which is what we would expect. Some levels are quite high, particularly in one location (Algonquin, Ontario) which is an area sensitive to acid deposition (acid rain). One of the purposes of the survey was to determine whether acidification of the environment was related to increased accumulation of cadmium in wild herbivores. There was not a clear-cut indication that this was true, but the Algonquin sampling site, which was a non-buffered location, did have the highest levels of cadmium. Most of the young animals had very low cadmium levels, but as age increased, the cadmium levels also increased (Table 3-7). Even in the older animals, kidney cadmium concentrations were not toxicologically high from the point of view of the health of the animals.

Table 3-7. Bioaccumulation levels of cadmium in moose kidneys as a function of animal age (Cd concentration in µg/g wet wt.).

Calf	3.0 ± 0.4 (17)
Yearling	12.8 ± 1.9 (16)
Young adult	20.3 ± 2.1 (34)
Adult	51.4 ± 5.3 (14)

Source: From Glooschenko *et al.* 1988

From the point of view of exposure to humans eating the tissue, however, there have been health advisories issued in Ontario against human consumption of kidney and liver from moose and deer. The advisories are based on WHO/FAO provisional tolerable weekly intakes of cadmium in food (WHO 1972), which would be exceeded if people were habitually eating these tissues. The advisory does not apply to the muscle tissue, which has low levels of cadmium. Similarly, moose and deer from Quebec, and also caribou from northern Quebec, demonstrate increased cadmium concentrations with age (Crete *et al.* 1987, 1989). Again, health advisories have been issued. Hunters are advised not to consume liver or kidney tissues from these animals.

High levels of cadmium are sometimes found in certain marine animals as well; for example, seals from various locations across arctic North America and Greenland (e.g., Goldblatt and Anthony 1983). Some of these seals have levels which are toxicologically significant, levels that are approaching values at which we would expect to see kidney toxicity occurring. For some time, it wasn't understood why these levels were so high because there was no obvious source of contamination by cadmium. We now know that certain marine invertebrates tend to accumulate cadmium even in the absence of environmental contamination by cadmium. For example, there are certain classes of marine amphipods that accumulate cadmium (Macdonald and Sprague 1988). These are pelagic species, yet they are accumulating 15 to 25 ppm of cadmium. Most other arctic marine invertebrates have quite low cadmium concentrations. It happens that these amphipods form a major food source for seals at certain times of the year when cod are scarce. Because seals eat high numbers of these invertebrates for long periods of time, over many years, they tend to accumulate high levels of cadmium in their kidneys. Thus, in certain food chains there may be bioaccumulation of a toxic metal such as cadmium even when there isn't any particularly obvious source of environmental contamination by that metal. Contaminating the environment with the metal may exacerbate the accumulation.

A similar phenomenon of high tissue cadmium concentrations has also been reported for certain pelagic seabirds (Bull *et al.* 1977; Table 3-8) collected in various locations in the tropical Atlantic Ocean. The species that had the highest kidney cadmium levels were fulmars and shearwaters, with concentrations up to 230 to 240 ppm dry weight. This is very much higher than one would expect in wildlife from a non-contaminated environment. As was discovered in the Arctic, there are certain tropical pelagic invertebrates – in this case sea skaters – that concentrate cadmium from the marine waters that they live in. They accumulate quite high levels, up to 300 ppm in one particular specimen. The Canadian Wildlife Service has recently taken part in a similar survey of seabirds from Atlantic Canada. It was found that certain species (storm petrels) accumulated high levels of cadmium. Several kidney cadmium values over 200 ppm (dry wt.) and a few over 300 ppm were detected. So again, here is a situation where organisms are accumulating a metal that one wouldn't particularly expect them to under the circumstances. It is important to be able to spot these kinds of situations when they occur.

Table 3-8. High cadmium levels in fulmars, manx shearwaters, and their (potential) insect prey, sea-skaters (Cd concentrations in $\mu g/g$ wet wt.).

Fulmar (kidney)	up to 240
Manx shearwater (kidney)	up to 231
Sea-skater (Halobates)	average 23 up to 300

Source: From Bull et al. 1977

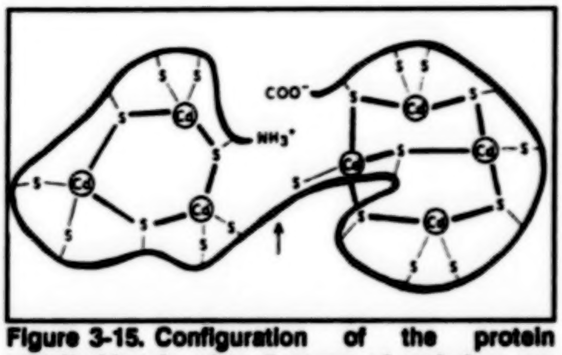

Figure 3-15. Configuration of the protein metallothionein when 7 atoms of cadmium are bound to it.

Most of the cadmium in tissues is bound to a specific protein. The protein is called metallothionein. It is a fairly small protein, about 6500 daltons molecular weight. An interesting feature about it, from a biochemical point of view, is that metallothionein is composed of about 30% cysteine. This means that the protein has numerous -SH groups. When cadmium enters a tissue, it induces the synthesis of this protein, which then binds the cadmium. Figure 3-15 shows the configuration of the protein when cadmium is bound to it. Cadmium is bound in two clusters; a four metal cluster and a three metal cluster. A maximum of seven cadmium atoms are bound to each molecule of metallothionein. As cadmium accumulates in the tissue, it accumulates primarily in this form, bound to metallothionein. The synthesis of metallothionein and the subsequent binding of cadmium to the protein is generally assumed to be a detoxification mechanism, by which cells can sequester the cadmium and prevent it from having detrimental effects. However, once the levels of cadmium reach between 100 to 200 μg/g wet weight in kidneys, either the capacity of the cells to produce this protein or some other threshold is reached, and the cadmium begins to have toxic effects. We generally assess cadmium levels in a tissue using this criteria. If the levels have reached 100-200 ppm (wet wt.), then there is a high potential for renal toxicity to occur.

Ray Emerson: I assume that there is some mechanism for other cation inducers?

Anton Scheuhammer: Not necessarily. Some other metals are good inducers of metallothionein, others are not. Cadmium is the best. Zinc induces as well, but in mammals and birds, zinc is not generally a toxicological problem. It is an essential trace element, and it is homeostatically controlled. So you can have an increased exposure to zinc in the diet and it is probably not going to have a great toxic potential.

There are relatively few metals that associate with metallothionein and which can effectively induce its synthesis. Cadmium is the major non-essential heavy metal, and zinc and copper are the major essential trace metals that are involved. Inorganic mercury can also induce metallothionein synthesis. But of the metals that would normally be associated with the protein, the one of greatest toxicological concern is cadmium.

Ray Emerson: Well, the protein in its normal state is without the cadmium, right?

Anton Scheuhammer: The protein in its normal state, in the sense that you mean, essentially doesn't exist. It is not synthesized unless there is some influx of metal into the cells. So as soon as there is such an influx of metal, the synthetic process starts up and this protein is synthesized and then it starts to bind the metal. So you never find the protein in its "apo" form. Naturally occurring apothionein doesn't really exist. You can make it in the lab by isolating the protein and stripping the metals off.

Ray Emerson: The DNA must be producing the protein and that is triggered by the cadmium.

Anton Scheuhammer: Yes, that is exactly right. There is a great deal of ongoing research being conducted on the molecular processes that occur in the induction of this synthetic mechanism. Metallothionein has been isolated and characterized in a wide variety of organisms; invertebrates, fish, reptiles, birds, mammals. It seems that virtually all organisms have the ability to make this protein.

Tom Kosatsky: What is the bioavailability of cadmium?

Anton Scheuhammer: Cadmium bound to metallothionein is probably less bioavailable than ionic cadmium, but how much less I don't know. If I had a choice between drinking a solution of cadmium chloride, or eating a piece of meat that had cadmium biologically incorporated into it, I would probably choose the meat. The gastrointestinal absorption of cadmium is somewhat less when it is bound to metallothionein.

Figure 3-16 shows that tissue levels of metallothionein follow the levels of cadmium. The metallothionein levels reflect the degree of exposure to cadmium. Thus, one can use metallothionein levels as an indicator of cadmium exposure, and it provides a measurement of the magnitude of the biological response to cadmium exposure.

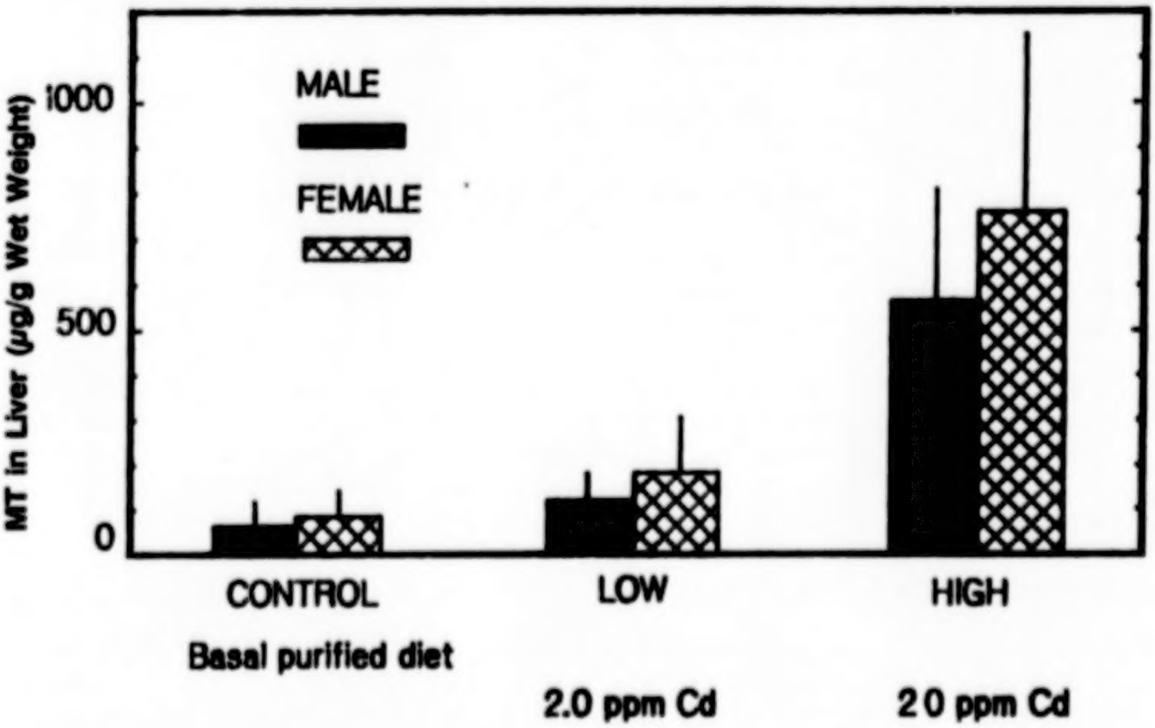

Figure 3-16. Metallothionein levels in ring dove livers exposed to different levels of dietary cadmium (concentration in µg/g wet weight).

LEAD

Lastly I would like to speak briefly about lead. I am going to restrict my discussion to inorganic lead, because although there are some highly toxic organic forms of lead, they are almost exclusively products of industrial activity, such as the organolead compounds manufactured as antiknock agents in leaded gasoline, and thus they are not likely to be relevant to the proposed dredging operation.

As is usual for inorganic metals, the absorption of lead from the gastrointestinal tract is quite low (a few percent of the ingested dose). Absorbed lead distributes primarily to bone. Most of the body burden of lead in our bodies, and that of other vertebrates, is localized in the skeleton. Of the soft tissues, kidney and liver tend to have the highest levels. In bone, lead has a very long biological half life (many years). It has a very much shorter half life in the soft tissues (3 to 4 weeks).

Female birds tend to accumulate higher levels of lead in bone than males fed diets containing comparable levels of lead (Figure 3-17). When female birds lay eggs, they mobilize calcium from their bones. They then increase the absorption of calcium from the diet to replace the calcium lost in making egg shells. When they increase their absorption of calcium, they also increase their absorption of lead. Another factor that influences lead uptake is the calcium level of the diet (Figure 3-18). If the dietary calcium levels are high (3%), the lead accumulation in bone is quite low in both males and females. When dietary calcium levels are reduced (0.3%), lead accumulation increases. Organisms physiologically adapt to a lower dietary calcium level by increasing the absorption of calcium, and along with calcium, increased levels of lead are also accumulated. Thus, certain nutritional factors drastically influence the accumulation of lead from dietary exposure.

Toxicologically, lead mainly affects the blood system, the brain, central, and peripheral nervous system, and like cadmium it can also be toxic to the kidney. The most commonly used indicator of exposure to lead is the blood-lead concentration, which is generally expressed as µg/100 mℓ blood. When the levels are less than 40, one generally tends not to see classical clinical symptoms of lead poisoning. More recent studies indicate that lower levels of lead in blood of children (~20 µg/100 mℓ) can be associated with effects on the central nervous system, resulting in cognitive dysfunction. But with respect to the development of the classical signs and symptoms of chronic lead intoxication, it is when the lead levels in blood are between 40 and 80, that the toxic effects of lead start to become manifest. There is interference with heme synthesis and urinary amino levulinic acid excretion increases. There are more immature red blood cells in circulation. There is a decrease in the efficiency of nerve conductivity. Renal inclusion bodies are formed. These are lead/protein complexes that accumulate in the nuclei of cells, particularly in the kidneys. They are a hallmark of increased lead exposure. Once lead levels in blood are >80, irreversible lead intoxication may occur. There is anemia, renal dysfunction, and the signs and symptoms of the neurological effects of lead. Thus, blood lead concentrations can be used to indicate the risk for developing lead toxicity. Also, the inhibition of a particular enzyme in blood, amino levulinic acid dehydratase (ALAD) is very closely correlated with the degree of lead exposure, and with the blood lead concentration. This enzyme requires the presence zinc and a thiol reducing agent in order to be fully active. When zinc plus dithiolthreitol (a sulfhyryl reducing agent) are added to lead-exposed blood, we can recover virtually 100% of the enzyme activity. By measuring the apparent, non-activated activity, and then restoring the enzyme by adding zinc and dithiolthreitol to get the total activated level,

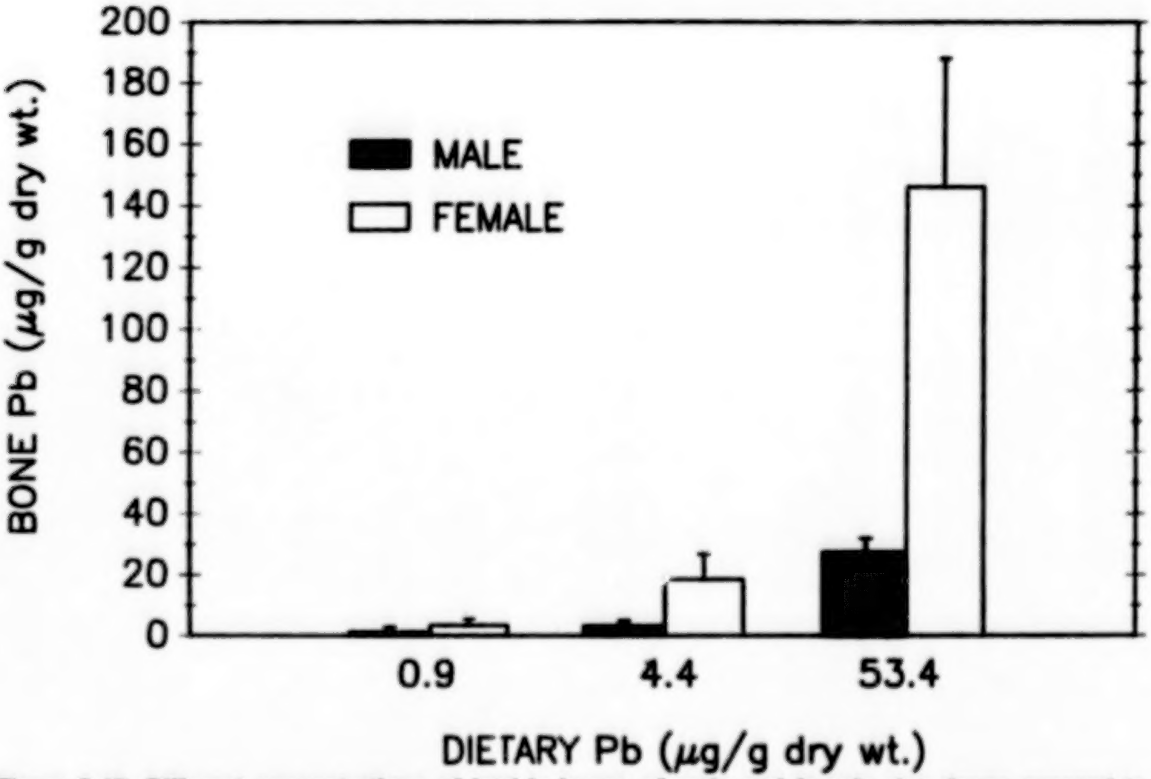

Figure 3-17. Different concentrations of lead in bones of male and female ring doves exposed to different dietary concentrations of lead.

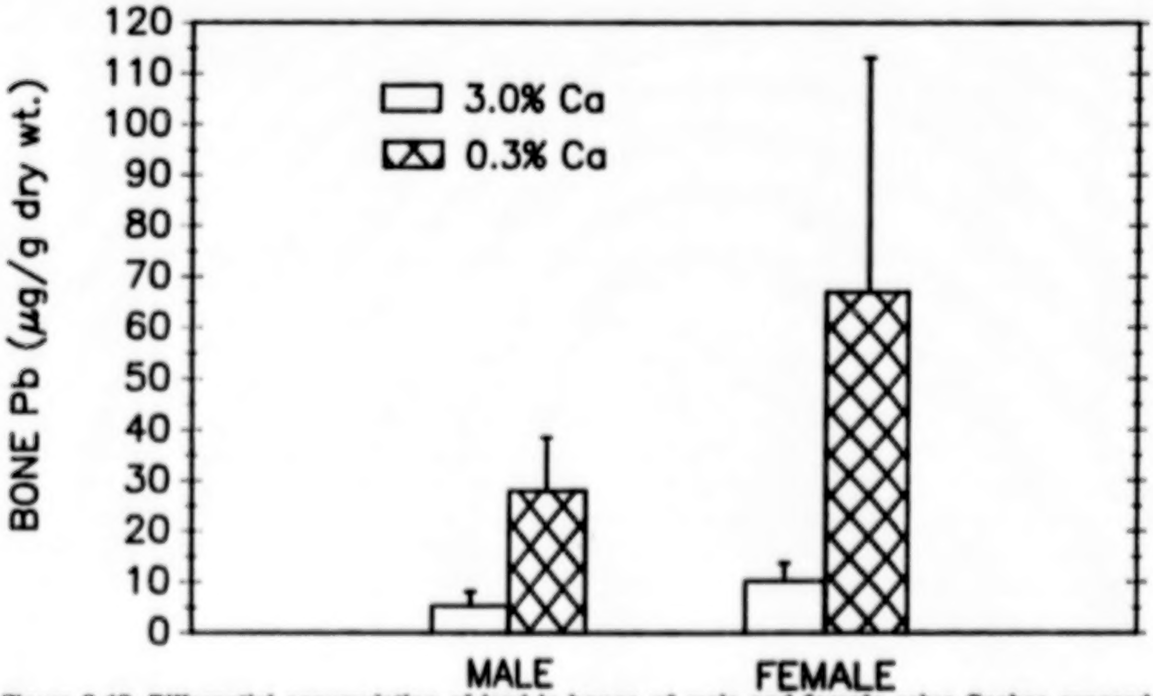

Figure 3-18. Differential accumulation of lead in bones of male and female zebra finches exposed to different levels of dietary calcium plus 10 ppm lead in drinking water.

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we can determine the relative degree of lead exposure. When there hasn't been excessive lead exposure, the ratio between these two values is approximately one; the activities are almost the same. But if there has been increased exposure to lead, the ratio of activated:non-activated increases dramatically. The ALAD activity ratio is a very sensitive, convenient indicator of blood lead levels and lead exposure in wild animals (Scheuhammer 1989b).

That completes my presentation on the toxicology of mercury, cadmium, and lead. As I said, I shall not be discussing the toxicology of other metals that seem to be of concern at this workshop; for example, arsenic. I have not studied arsenic and any questions regarding arsenic should be directed to Dr. Eisler. However, I am willing to entertain comments or questions regarding cadmium, lead, or mercury.

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FOOD CHAIN AND BIOACCUMULATION PROGRAM SESSION

RECOMMENDATIONS

FOOD CHAIN AND BIOACCUMULATION PROGRAM SESSION

SUMMARY OF RECOMMENDED STUDY ELEMENTS

Assumptions

- That no mercury is used on dredge or will be released from it.
- That there is no information regarding Cd, Cu, Hg, Pb, Zn, As, Cr, or Ni in deep (0-3.6 m) sediments of the study area.

Recommended Bioindicators (Specify units; that is, fresh weight, dry weight, or lipid weight)

1. Benthos:
    a. Filter-feeding bivalve mollusks, *Spisula* or *Tellina*
    b. King and tanner crabs, hepatopancreas
    c. Brittle stars

2. Fishes:
    a. Tomcod - muscle
    b. Alternatives: Starry flounder, sculpins

3. Birds:
    a. Murre - eggs, muscle and liver, Hg and Cd
    b. Cormorants - eggs, Hg
    c. Peregrine falcon - feathers, eggs, Hg

4. Mammals:
    a. Ribbon seal - muscle, blubber, Hg and Cd
    b. Bearded seal - liver and kidney, Hg and Cd

Sample design - Benthos
1. Experimental - 500 m from dredge, 3 stations
2. Central - 2500 m from dredge, 3 stations
3. Frequency - prior to, during, and after dredging

Sample design - Caged mussels
1. Experimental - 500 m "downstream", surface, mid-depth and near-bottom
2. Control - 500 m "upstream", surface, mid-depth and near-bottom
3. Frequency - Prior to, during, and after dredging
4. Relocate - Arrays as barge moves to maintain fixed distances

Related Concerns
1. Should integrate species diversity, abundance, and biomass from habitat alteration group to identify effects on food chain dynamics of crabs.

2. Promote coordination between this monitoring program and ongoing efforts such as NOAA's mussel watch and status and trends; and MMS's seabird tissue analysis and seabird colony monitoring programs.

FOOD CHAIN AND BIOACCUMULATION PROGRAM SESSION

REVIEW AND RECOMMENDATIONS

Ronald Eisler: Our first concern was whether liquid mercury will be used in mining operations. I've been assured that no mercury would be used; however, the last draft addendum indicated that 0.3 lb of mercury would be discharged into the environment daily and that the barge would carry 2,035 kilograms of liquid mercury. If mercury is used, recommendations that follow need to be modified drastically.

We perceive a data gap concerning the chemical analysis of sediments and sediment interstitial waters. The recommendation that surficial (5 cm) sediments should be analyzed is useful, but I believe that we should also sample sediments from 3.6 m. It's quite possible that chemical analysis of sediment interstitial waters are below detection limits. At this point, it is recommended that the sediments and the interstitial waters should be analyzed for the entire suite of 123 EPA Priority Pollutants. Only in this manner can an informed judgment be made. This is a perceived data gap.

In the absence of any other information, we selected a suite of metals that are known bioaccumulators and have potential adverse effects. Cadmium, mercury, and lead present significant problems to upper trophic level organisms as well as to lower organisms. Copper and zinc are demonstrably toxic to certain important constituents in the ecosystem at extremely low levels. A lower priority would include arsenic, chromium, and nickel, and various organics present or measured in sediment interstitial waters. In addition to PAHs, PCBs, and pesticides, a number of other components might be detected.

We also recommend bioindicator organisms from different environmental compartments, for one, filter feeding bivalve molluscs. It is known that *Spisula* and *Tellina* occur in Norton Sound; whether they occur in sufficient quantity for analysis is not known. Animals of approximately the same size must be used.

Hepatopancreas of king crabs and tanner crabs are recommended tissues. Crustaceans, in general, aren't good metal accumulators, but the hepatopancreas is known to accumulate cadmium and many organics. Another recommended bioindicator group were the echinoderms, because brittle stars are important in the food chain of king crab, and known to be locally abundant.

Concerning fish, Saffron cod muscle is recommended. This species is taken at the end of the dredging season, in September, but its migratory habits are unclear. However it is an indication of baseline condition. Starry flounder and sculpins were suggested, but these were assigned a lower priority.

For birds, we recommend analysis of mercury and cadmium in eggs, muscle, and liver of the murre; mercury in eggs of the fish-eating cormorant; and peregrine – an endangered species – mercury in feathers. The Endangered Species Group collects peregrine eggs every five years or so; this sampling should be incorporated into the recommended sampling scheme where possible.

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Two species of mammals were suggested: the ringed seal, which occurs in very large quantities and is harvested in Norton Sound; and the bearded seal, which apparently has a different kind of diet. Again, cadmium and mercury in various tissues – muscle, blubber, liver, and kidney – seem to be of concern.

Chemical analysis of samples should clearly stipulate whether sample is fresh weight, dry weight, or lipid weight. It is distressing to learn that there is "X" ppm of mercury in the liver and no information given on whether it is a dry weight basis or fresh weight basis.

The sixth point concerns the proposed sampling design. One item that this group suggests – although perhaps this should be deferred to the biometrics group – in terms of the benthos was a total of six stations, triangulating the barge: three, 500 m from the barge, and three 2500 m from the barge. That is assuming that the barge doesn't move large distances, as could be the case. These stations would be sampled for the various indicators just prior to dredging, mid-season, and after dredging. This could be coordinated with the collection of sediments and interstitial waters.

The group had a lengthy discussion on cage studies, and again we defer this to the statisticians. We recommended floating cages; one 500 m downstream from the effluent [one at the surface, one at mid-depth, and one at the bottom] and another set 500 m upstream. Sub-samples would be collected periodically, probably according to a logarithmic sampling interval. The longer the array is deployed, the greater the chance that something will go wrong. Some test organisms will be eaten, others will become diseased.

If the barge moves substantial distances, the cages should be set fore and aft. The sampling frequency would be just prior to dredging, mid-season, and immediately after the mining season is closed. When the caged mussels are sampled, there is an opportunity to measure their growth and to observe gross pathology, survival, and perhaps reproduction.

In a dredging situation, there will be perturbed communities that will be resettled by different types of organisms. Although the same amount of biomass may be produced, the question is whether the new community is representative of the old community in terms of, for example, food chain dynamics of the two commercially important species of crustaceans. WestGold conducted microbenthos studies, but perhaps the macrobenthos should be examined.

Finally, and this an important consideration, there are many groups working in Norton Sound and each has a program. All panels and groups should coordinate with other panels, and with ongoing programs, such as NOAA's Mussel Watch, NOAA's Status and Trends, and the Minerals Management Service's Seabird Tissue Analysis Program and any seabird colony monitoring.

It is clear that this Panel has addressed problems that are of concern to other panels, with sediment, water quality, habitat alteration groups.

Ray Emerson: I have one quick question concerning differences, between your talk, Ron and the last one. I guess maybe my question is directed more to Eric: How did you folks arrive at the number of samples? Ron is deferring to the statisticians, in terms of number of samples, but you came right out and told us the exact number. What's the basis for that?

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Eric Crecelius: The initial estimate ranged from 1 to 25, so we went about halfway. We wanted to get samples from two different lease areas and we wanted to cover the three main grain sizes expected to be found in that area. So we felt the minimum would be to sample 3 distances offshore (which are supposed to correspond to three different grain sizes) and to look at the depositional and non-depositional areas in each of the two lease areas. Then we need to have some background stations outside those areas. This is the minimum you would need in order to say that changes have occurred in surface sediment.

Ray Emerson: Were you expecting to do the four transects in one day, or two, or was that considered? Did you assume a good day in Norton Sound?

Eric Crecelius: We haven't taken into account how many ship days it would take to collect the samples. I'm guessing that it would be possible to do this whole thing easily in a week. I'm sure you can't do it in one day, but I think you could do it in a week.

Ray Emerson: My last question was regarding arsenic. You didn't have arsenic in your suite, did you?

Eric Crecelius: Yes, arsenic is one of the six metals that will be done in all sediments, and in all water and all interstitial samples too.

Patrick Scannell: Without getting into a debate on particular components, I'd like to pass on a couple of things. First, a background survey was conducted this spring on peregrine falcons. Samples were collected and they've been shipped back for analysis, so we do have a starting point there.

Second on the caged mussel program, some caution might be in order. A distance of 500 m for both upstream and downstream stations, will place both stations within the plume, since the tidal current fluxes both ways. Based on our experience in the inshore waters, you're going to be in the dredge plume.

Ronald Eisler: Thank you. Any other questions?

Donald Hood: Do you have any problem with the logistics plan that Eric outlined?

Ronald Eisler: No. We tried to keep our sampling frequency to a minimum, and in fact, even factored in graduate students and summer employment to keep this a cost-effective method of obtaining data. I think it could all be done with a limited number of senior investigators and many graduate students. And it could be done along with the water quality and sediment group. There's no reason to charter a boat for a second day if the same stations can be used.

Woollcott Smith: Well, there is a definite conflict because your sampling plan envisions three sampling times a season and Eric envisions one. There is a logistics conflict there.

Donald Hood: Yes, but the biological organisms have to be monitored...

Woollcott Smith: I agree with you, I'm just pointing out that the schemes are in conflict...

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Gail Irvine: I have a question about the use of mussels. I can understand the rationale for moving mussels behind the barge. But that depends on your intent; whether to get the amount of metals an *in situ* mussel would be exposed to, or whether you're trying to get a peak amount.

Ronald Eisler: We're trying to determine the rates at which mussels accumulate. The WestGold data indicated essentially negative accumulation: a) Their mussel cages were 1000 m distant, and did not incorporate tidal fluxes. It would be helpful to account for Norton Sound hydrogeography; and b) WestGold studies were conducted when the water temperature ranged between -10° to 10°C. Accumulation rates are low or negligible, although growth was possible at these temperatures. Water temperature strongly influences accumulation rates and patterns in bivalve molluscs and should be incorporated into any sampling design.

Gail Irvine: But it's not indicative of what organisms in that environment actually face, since you're moving these around and exposing them to the peak exposure, and trying to approximate the peak.

Ronald Eisler: The group discussed this point. Ideally, a sampling grid would have 200 stations, and all would be analyzed. However, we didn't realize that the barge moved frequently. In a fixed case the barge would be triangulated with six stations: three at 500 and three at 2500 m. But as the barge moves, those stations become inoperative, and the need is reestablished for another six stations.

We believe that the experimentals should always be a fixed distance from the barge, and this will maximize exposure effects. A distance of 500 m is reasonable and proper, although some federal regulatory agencies suggest 100 m. If the cages were directly under the outfall, excessive silt would kill all the biota.

The group believes that some biological damage will occur in Norton Sound as a result of mining operations, but we think it will be minimal. We just want to be sure that the types of chemicals that are going into the system are measured, or at least have been analyzed to see if they're worth measuring.

Gail Irvine: Since you recognized that these mussels may not be very active metabolically at those temperatures, do you think it's worth doing at all? How can you say that you're expecting to get peak maximal exposure when you are using organisms that may not be metabolically active?

Ronald Eisler: It's not peak maximal exposure, it's optimal exposure. Mussels do not integrate as well as clams such as *Spisula* simply because they can seal themselves off under adverse conditions. But they're better than oysters. The point is that large quantities of mussels can be obtained from a common source, and these can be subsampled. I don't think that this can be accomplished for other species, except, perhaps, softshell clams. There's also a large database on mussels.

Yes, I think it's worth doing. No, I don't know what to expect. Incidentally, as far as sea birds are concerned, Dr. Harry Ohlendorf of the U.S. Fish and Wildlife Service in Davis, California, reviewed contaminants and sea birds in 1975. He is updating this review, and it

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should be available in February 1990, and will include inorganics, and pesticides. The applicability of this review to Norton Sound should be considered.

Mac McLean: Nobody wants to fall on their sword over the distance from the barge, but that was a factor we considered when we set things up last summer.

A couple of additional points: 1) The temperatures may be low, but they are the ambient temperatures. That's the range that one has to deal with, and the range that any other critter out there would be exposed to, as far as metabolic activity goes. 2) We were also concerned (because there is a very discernible suspended sediment load at 500 m) that we would get a shut-down on feeding. We wanted to maximize feeding rates in order to maximize concentration. Somewhere there is an appropriate balance and one has to try and figure out exactly where. The third factor has to do with the tidal current patterns. The turbidity plumes that we've seen from the existing BIMA operation tend to spiral 360°, all the way around. It is not just simply in-and-out, a back and forth flushing. The logistics of trying to keep a caged mussel program going, when it sweeps 360° on a daily basis, and when you've got variable weather conditions, are overwhelming. It creates a problem with trying to use that type of approach.

Ronald Eisler: An additional series of cages 2000 m away may compensate. And we may have to consider that the aft station may be more impacted than the fore station, and compare these to groups that are even farther away. I don't know what the maximum water temperature is. Up until July it was 10.1°C, but does it get higher? Every tenth of a degree over that makes a critical difference to uptake.

Mac McLean: 10.1°C is just about maximum. I haven't really thought this through, but one possible way to approach it might be a cage using introduced sediment, where you can hold some variable constant, and just use the mussel as an integrator.

Ronald Eisler: I don't agree, but we can discuss this later.

Bryan MacLean: Can I muscle in and make my comment here? Then I'll clam up. The human health group was concerned, among other things, with the level of trace metals in subsistence resources, with bioaccumulation, and with the effects on the end user. I noticed you had murres as one indicator and tomcod as another. Did I miss marine mammals? Because there was some speculation --

Ronald Eisler: Yes, you missed ringed seals and bearded seals.

Gail Irvine: I think I was misinterpreted about the metabolic temperatures at which these animals operate. I'm not arguing that they're not exposed to those temperatures, it's just that they would be transplanted from a warmer environment to a colder one. I don't think that is representative of how organisms from Norton Sound operate.

Ronald Eisler: Are there any species of bivalve molluscs that occur in sufficient abundance that several thousand could be obtained at one time?

Gail Irvine: Well, if you were as efficient as a walrus, maybe you could get some clams and use those. That may be possible around Sledge Island, where there are concentrations of clams.

Ronald Eisler: The problem is using a species other than *Mytilus edulis*. You could use another species in addition to *Mytilus*, but *Mytilus* is now part of our folk culture, you might say. I have no idea how old a 30 mm mussel is, i.e., the length WestGold used. Off Florida, that would be a year to a year and a half old; it may be 10 to 20 years old in Norton Sound.

Gail Irvine: You could probably age them, using the lining in the shell, but that's not the only point.

Ronald Eisler: The purpose of this monitoring program is to determine whether a gross, adverse effect is occurring or may occur. Nevertheless, an impartial measure of effects or potential effects must be implemented. No such scenario is evident.

Gail Irvine: I agree with you. It's just that I'm now concerned whether that's the most appropriate organism and the method of how you'd place it.

Ronald Eisler: This group suggested 500 m, while EPA recommends 100 m. To maximize effects, organisms should experience what comes out of the end of the pipe. If the organism can't live in that outfall water, then that outfall water has to be cleaned up. But there are allowances, there are perturbations; many events are occurring simultaneously. There are 178,000 acres in the lease sale but only 200 or 300 acres are mined each year. At present, we see no evidence of complete recovery two years after dredging in the inshore environment. This environment is extremely unstable compared to the offshore environment. I suspect that the offshore environment will recover much more quickly than the inshore environment.

Gail Irvine: I could also say that you might expect the offshore to recover much more slowly, because of the adaptations of inshore organisms for exposure to disturbances. But that's another hypothesis.

Ronald Eisler: I'm talking about a return to the original community in the inshore environment. Although recovery of biomass occurs, it isn't the community that was present initially. Numerous opportunist species occur in inshore environments compared to offshore communities.

Malcolm Robb: I'd like to make a comment, because we seem to be talking about two different things. The water quality group described a baseline study some years prior to mining, whereas you are describing a monitoring plan that can only occur once you know when mining is to start. These are two mutually exclusive studies, and there's no logistic conflict. Maybe we should be talking about the same study.

Ronald Eisler: That is a salient point. How do you measure the effects of a moving target? It is relatively easy to measure a point source effect: all cage studies with mussels that have been conducted under 301(h) by EPA are designed for open-ocean sewage outfalls. But this is more challenging: even if the cages following the moving target, how does one account for background levels?

Malcolm Robb: My point is that this is not part of the baseline study. I thought the purpose of this meeting was to design a baseline study. You won't be able to do this study until you have a lessee in place with a dredge and that's some years down the line. That is a different subject than what the water quality group talked about.

Ronald Eisler: You said that before dredging, you're going to have to test your methods and your analyses. So I see at least one or two summers in which you can do some preliminary analysis, and perfect your techniques. You're just not going to get your crew together and say, that they're starting to dredge tomorrow and that we have to collect today. If you haven't prepared by taking a number of samples prior to this...

Malcolm Robb: But you're not going to know where they're going to dredge, not to within 500 m or 1 km or even 10 km.

Ronald Eisler: We will at the beginning of the dredging season, at least.

Malcolm Robb: Well, that's at the beginning of the dredging season, not during the baseline survey.

Ronald Eisler: In terms of sediments and biotic organisms, the baseline survey needs to establish the minimum number of samples from the lease sale area and outside the lease sale area, to get variability.

Malcolm Robb: Well, that is completely different from what you've outlined on the screen.

Ray Emerson: Can I maybe make it more complicated? I think we are trying to cover two aspects here, and I agree with the person that just spoke, that (this) is a dredging activity. We are setting up the baseline conditions, but we don't have bioaccumulation under the baseline condition because we don't have the dredging yet.

When we do have the activity that feature will go into place. I think the speakers are addressing both of those aspects. In other words, we're looking at a baseline to monitor from, to determine changes. So we've got both of those aspects that we're considering in this workshop.

Cleve Cowles: The concept of baseline is a concept of time; as you mentioned, you have a pre-dredging exposure, a during, and a post. The "pre" constitutes a baseline, it's just a very short time interval compared to what you might have in another discipline.

Anton Scheuhammer: Also, I would like to point out that it's only cage studies that are contingent upon the dredging operation. The other proposed sampling stations can be set up whether there's a barge or not, and could be run for two or three years before any dredging occurs. Once dredging begins, we would sample directly before the dredging, during the dredging, and after the dredging is over for that season.

Dick Prentki: I'd like to comment on the distances of your control samples, in relation to your baseline work too. The water quality section thought a safe distance for a control sample was probably about 10 km from the dredging site. Anything closer than that may get some effects. A study in southeast Asia, where they used a BIMA-like dredge for tin mining offshore gravels, showed an effect of the turbidity on phytoplankton up to 5 km away. So, you may need to move your control distance out quite a bit.

Ronald Eisler: We were informed that the plume might extend 1000 m. That is why we selected 2500 m for the second series of stations.

Dick Prentki: That distance is for 95% of the time.

Donald Hood: Does anyone want to see that plan again? It does seem to me that the discussion here is centered on the mussel accumulation problem, which is before, and during, and after dredging. I think it would be good to see how this group would compare to the water quality group, on what the water quality group calls the baseline study. Would that be helpful?

John Trefry: Well, let me share with you some of the comments that the water quality and sediment group made about the "longer cores." It seems the charge that we've been given is to detect change, not to predict change. And so, given that approach, I don't think that long cores are necessary. We focused on surface sediments because that's the place we're going to detect change.

With respect to deep interstitial water, we did a series of calculations using worst-case interstitial water concentrations. The interstitial water will be so effectively diluted on board the barge, just by the seawater that's brought up, that it will barely be discernible above background level. That was the reason the water quality group stuck with surficial sediments. Costwise and detection-wise, that's probably the best mode we came up with.

Ronald Eisler: If you find organics in the deep sediment interstitial waters, some may be bioaccumulated to a very high degree. If you had three football fields end to end, filled to a depth of 12 ft, and you added one teaspoon of DDT at one end, that's enough to kill 50% of the fish life. That is a powerful effect. These pollutants can really spread out and can be taken up far more than you can imagine, and I, for one, would like to know what it is that we're dealing with in the lower sediment interstitial waters.

John Trefry: Well, I certainly agree with the idea of longer cores, but again, the admonition we were given was to detect, not predict. I think we are forced to look at surficial sediments.

Anton Scheuhammer: Well, our group (Food Chain/Bioaccumulation) came to the consensus that deep interstitial sampling should be done on a one-time basis. This is not part of the background monitoring and it's not really part of any ongoing monitoring program. It was simply to identify other substances that we might want to include in the monitoring program. Ron was concerned that there might be other metals of concern that we simply don't know about. There might be organics that we simply don't know about. We felt it was wise to do two or three of these cores to fill that information gap on a one-time basis.

Eric Crecelius: In the remote possibility that we see some unusual chemicals in the mussels or benthic organisms, after dredging has commenced, we could use an alternative approach. We could always go back and collect cores from non-dredged areas and do the interstitial water chemistry to see if that is a source. The interstitial water is always going to be available out there in the field. We can go back at a later time and search for sources.

Ronald Eisler: Why would you expect to find those unusual chemicals, if you're not looking for them in the first place?

John Trefry: If nothing unusual shows up in the tissues or there isn't any catastrophic biological change that you can't explain from natural processes, there's no reason to start looking further for unusual things.

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Ronald Eisler: Well, we still believe that this is a data gap.

Anton Scheuhammer: You see, we have not suggested measuring silver, for example, in the benthos. We don't consider that reasonable on an *a priori* basis. But maybe in this particular case, in this particular environment, silver is a potentially relevant metal to measure. There's no way we're going to know that, either in a baseline study or in a future monitoring study, if we have decided *a priori* not to measure silver. The only way we're going to decide whether we should add silver or any other metals, or any organics to this suite of things we want to measure, is to do an extensive, one-time survey. That's all we're recommending here; I don't see why that's such a difficult concept.

Donald Hood: It looks like we have a bit of an argument here, but not too much difference in the outcome. I wouldn't think it would burden anyone too much to do interstitial waters in a core. I would also think that the only organics you could conceivably get in Nome are ones we already know are worldwide, PCP and DDT which travel by air. There is very little opportunity for any of the other powerful organics to get to Nome in a significant amount. Inorganics, perhaps yes, and it would seem worthwhile to have data on those. But Eric, that large suite of samples you were taking, didn't that earlier data include most trace metals, except not from interstitial water?

Eric Crecelius: In our first year of the baseline survey, some of the sediments would be analyzed for all EPA Priority Pollutant metals. And the fine grain sediment – from the finest grain, deposition area – would be analyzed for some of the EPA priority organic pollutants. This would be in the surface sediment.

Donald Hood: To get off this point of contention, I would suggest that it wouldn't be a bad idea to have some cores, at least to or three in the area, to provide at least strategic evidence of what the sediments were like before the dredge got there. It wouldn't be that much trouble to do interstitial water on those samples.

Eric Crecelius: The only objection I have would be the cost to bring in vibracoring equipment, a suitable barge, and a crane to do it. There is no problem doing it, it's just the $100,000 cost to get this stuff shipped up from Puget Sound to do it. Maybe $100,000 is much too inexpensive. But we commonly do interstitial water in major dredging projects in San Francisco Bay and Puget Sound. We even do bioassays with interstitial waters in areas where there are known high levels of contaminants and they are meaningful in those situations. So it's a question here of how much money you want to spend.

Donald Hood: I think you brought out a very important point. I was under the impression you might do it from the *Helix* with a vibracore and the cost wouldn't be so high – if the ship were already there. But certainly the priority for this would fall if it cost $100,000.

Don Hansen: When they explore for gold in the area, they do quite a bit of coring. I don't know whether that would facilitate some of this or not, but industry is already out there drilling many, many cores, there may be the opportunity to tag onto their program, to save money that way.

CHAPTER 4

HABITAT ALTERATION SESSION

Sathy Naidu, Ph.D., Session Chair
Jerry Kudenov, Ph.D., Assistant Session Chair

HABITAT ALTERATION

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By way of introduction to this session, I have some opening comments and remarks to make that I hope will facilitate the ensuing discussions and deliberations of this session.

Habitat, as you know, is a subtotal of characteristics of the environment resulting from the interactions of the physical, chemical, and biological attributes of the environment that are critical to the colonization and sustenance of biological communities in the environment. For the purpose of this workshop session, I suggest that our deliberations primarily focus on the offshore water column and benthic habitat within the Norton Sound federal lease sale area. I realize that any large scale operation within this lease sale area would have some impact on the onshore area, and the air quality within and in the adjacent areas. But for practical reasons, because of the limited time that we have at hand, I would like you to restrict your discussion to the offshore water column and the bottom sediments. I'm not quite sure whether we should include the seabirds that land on the sea surface. Are there any strong reasons to include the seabird habitat? Seabirds feed in this offshore area, and then fly back into their nesting grounds on shore. That is up to you to decide.

I would now like to move on to some of the pertinent questions that we should be asking. Questions that we need to address would be: Will there be any alteration of the habitat within the offshore lease sale area as a consequence of the exploration and exploitation processes relating to the placer mining operations? In order to address this important question, whether or not there would be any changes in the habitat, it is critical for us to have two basic inputs. The first is: What is the present habitat like? Do we have a sufficient baseline data in context of the inventory of the organisms that live within the Norton Sound habitat and especially data specific to the lease area? Do we know the trophic structure? Do we know the food web? These are very pertinent questions. Without knowing what the distribution pattern, densities and the community structure of the animals are -- what the population is like, what is the interaction between the different animals and their habitat, -- we cannot have meaningful discussion.

As far as I know there are some baseline data available. But these data are quite sketchy at best. There are two persons here, one is Gail Irvine, from MMS and the other person is Paul Rusanowski, from WestGold, who are in a much better position than I to provide you with what the status of knowledge of the different biological communities is within the area of concern. Gail, do we have a good database?

Gail Irvine: I think Paul will be able to speak with greater detail than I can, because due to their monitoring program for the WestGold operations, there's much more information in the nearshore environment than there is in offshore. We have, I would say, pretty sparse information for offshore areas. Usually programs are designed for oil and gas, where you're dealing with the whole of Norton Sound, and you have points spread throughout the Sound. So aside from a few particular studies where people have looked at feeding of walruses, and effects on clams, or something like that, where we have some site specific information, everything else is very

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sparse. So even understanding what type of habitat is present, or what the substrate types are, or what the distribution of substrate types within the area, it's not very well known.

Sathy Naidu: Okay. So we have to work with those limitations. Following my presentation, I'll ask Paul Rusanowski to give us an idea about what the present habitat looks like in areas adjacent to the federal lease area. The second basic input that we need is a knowledge of possible industrial activities that would occur within the lease area. We have an idea about some very obvious activities, such as dredging or discharge of tailings. So discussions can be based on certain assumptions or reasonable assumptions that you can make.

Will there be any alterations or impacts? The term impact generally seems to carry with it a connotation that is negative. A lot of people think of impact in terms of deleterious impact. So in your deliberations I would like you to look into the possibilities of, besides adverse effects, some beneficial effects that could result from the various industrial activities.

Now let me give you an example of how the dredging operation can lead to some beneficial effects on the habitat. In the Beaufort Sea, certain isolated areas have been identified with concentrations of gravel, chiefly of boulder size. The boulders appear to provide a unique environment supporting an unusually high concentration of a number of animal species and kelp beds. In fact, in the Beaufort Sea the so-called boulder beds are a unique ecological system. It is quite possible that the dredging operations may result in dumping of gravel and accumulation of large-scale gravel deposits at certain places, or mounds of them, that could eventually serve as an attractive area for colonization of a number of animals, sessile animals especially, and kelp.

Also, what we need to determine is whether the potential impacts will be of a major or minor scale, permanent or temporary? In the event of an adverse impact we need to look into the possibility of recolonization of the environment and the rate of colonization. A pertinent question to ask would be: how much time would it take for the original benthic community to attain a state of equilibrium, or the successional state it was at prior to an industrial activity.

Also, I would like you to look into the possibility of a change or restructuring in the biological community as a consequence of the presumed industrial activities. It is quite possible that the present benthic animals are mostly deposit feeders. Should the concentration of sediment suspension increase significantly as a result of the dredging operations, it is quite possible that there may result a restructuring of the benthic community, reflected in an increase in suspension feeders, relative to deposit feeders. So one benthic community can possibly give way to another benthic community consequent to the dredging operation and increased flux of fine tailings into the water column.

I urge you to put yourself in a mode of thinking, in context of a number of possible scenarios. Some of the scenarios are well spelled out in the draft EIS. So you need to go through a logical sequence of thoughts. Identify an industrial activity, and then follow up and try to determine the possible environmental impact in the context of the likely physical and chemical environmental perturbations.

If the federal lease area is dredged so extensively that it gets deeper, much deeper than it is now, would this operation in any way change the habitat? What would happen if there is a mobilization and discharge of natural chemicals during the dredging process? The present plan suggests that the dredging process will excavate the sea floor to about 3.5 to 3.6 m. If that were to be the case, then that large scale excavation would excavate the subsurface reduced layers, which generally have quite a relatively high concentrations of a suite of heavy metals in the dissolved state. The eventual result would be a discharge and mobilization of metals into the overlying aqueous environment, which could lead to possible deleterious impact on the biological community.

Also, we need to take into consideration the intensity of some of these activities. I understand that the initial dredging operation will involve one dredge, similar to what the operation is now. Later it is quite possible that the operations may be enhanced to include more than one dredge. Sometimes one activity on a very local scale may not alter the environment or the habitat significantly. However, if that activity were to be increased to more than one operation or involve a number of other sites, it is possible that there may be a cumulative effect. A phenomenon similar to that seen in the Beaufort Sea. Building of a single causeway there presumably didn't affect the environment much, but when more than one causeway was built, we could see the effect on the community, the large scale perturbation in the environment.

For the purpose of our discussions I would assume that the dredging would be sort of a bucket line operation, where a train of buckets on a line move up and down and excavate the bottom. If we assume that the dredge would be an analog to the one presently working, the BIMA, which would normally be in operation during the ice-free season, starting somewhere in June or early July, and ending in early September with the average depth of excavation of about 3.6 m, one can expect a total sediment excavation of about 1.5 m^3 within an area of about 100 acres. This calculates to 12,500 to 15,000 m^3 of sediment dredged per day.

This is a worst possible case scenario with the operation continuing for about 12 years, starting from about 1994. It is stipulated that the dredge would work within 20 to 30 m depth of water. These are some of the assumptions that you can take into consideration in your deliberations.

I don't say that the sky is the limit, you know, you have to be quite reasonable. Are there any questions? If none, I would like, to introduce Paul Rusanowski. Dr. Rusanowski is the manager of the environmental office of WestGold, the company that operates the dredge and is exploiting the gold offshore of Nome. I've asked Paul to give you a presentation, he's the man on the spot, in the field, and he should be able to provide you with answers to practical problems in the area.

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WESTGOLD'S ENVIRONMENTAL MONITORING PROGRAM

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I'd like to start off by making a couple of comments that are particularly relevant to keep in mind in terms of the topic of this three day session.

A lot of individuals here may not be familiar with conducting research or monitoring programs in the Arctic. For those that have not tried it, you should keep in mind that Murphy was indeed an optimist. If you don't believe that, start dabbling in Arctic research, monitoring programs, baseline studies, and it will make a believer out of you. An additional point to remember, relevant to this meeting, is that gold is where you find it. We don't control where the gold is. We can't optimize the locations. We have very little to do with where we are required to work to make a profit. So when you're designing studies to look at a process like this, establishing reference control areas, habitat types, standards of this sort, it's virtually impossible to predict what's going to happen. Even within a given year it's difficult to control the areal location where an activity would occur.

A good example of this is what we conducted in the early stages of the study. I've been involved it in for five years now. In the first three years we attempted to establish a baseline location that would be dredged by the mining operation, with virtual hundred percent certainty, so that we would have a "before" biological case to look at, and then be able to follow recolonization afterwards. In three successive years we missed even picking the best possible locations, waiting until just before the start of the dredging season. In spite of everything we tried, we were never able to locate a station that subsequently during the season got dredged. So that's just one of the things that you'll run into.

The mining lease is located in this general area, we have 21,000 acres. We have been in this area for three years.

The station I will focus on most will be R-6, that was dredged in 1986. The dredge footprint of impact that we're concerned with was roughly 28 acres and comprised roughly 24 acres of actual dredging and another 4 acres of burial with tailings.

We have our sampling program stratified offshore, so that we are looking at our first stratum being roughly 2 to 3 m, our second stratum 5 to 7 m, our third stratum roughly 10 m to 12 m, then a fourth stratum at 18 m. All of our sampling effort is distributed on that basis. We concentrate mainly in the third stratum. There's an important feature to remember in terms of moving offshore that I'll get into when we get to a few of the other slides.

You all recognize the BIMA (Figure 4-1). This is what it looks like when it's a great day, we don't get too many of those. Probably Matthew Iya is wondering what year that was taken, because we didn't see anything like that in 1989.

Quickly going through what we're doing: we pick up dirt off the sea floor with a bucket ladder dredge (Figure 4-2). It moves the material through a trammel. This is a washing screen, and we try and wash all that material that's in the buckets through this screen, so that it looks like this at the other end, just a little bit of water running out. If we're successful, that's what it looks like. If not, you'll see rocks and clay balls and all kinds of stuff coming out.

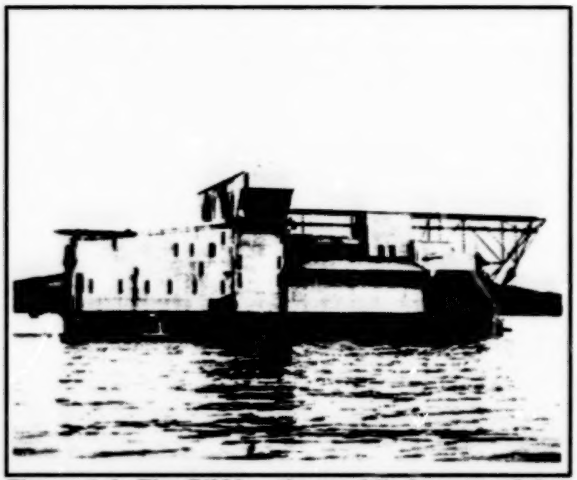
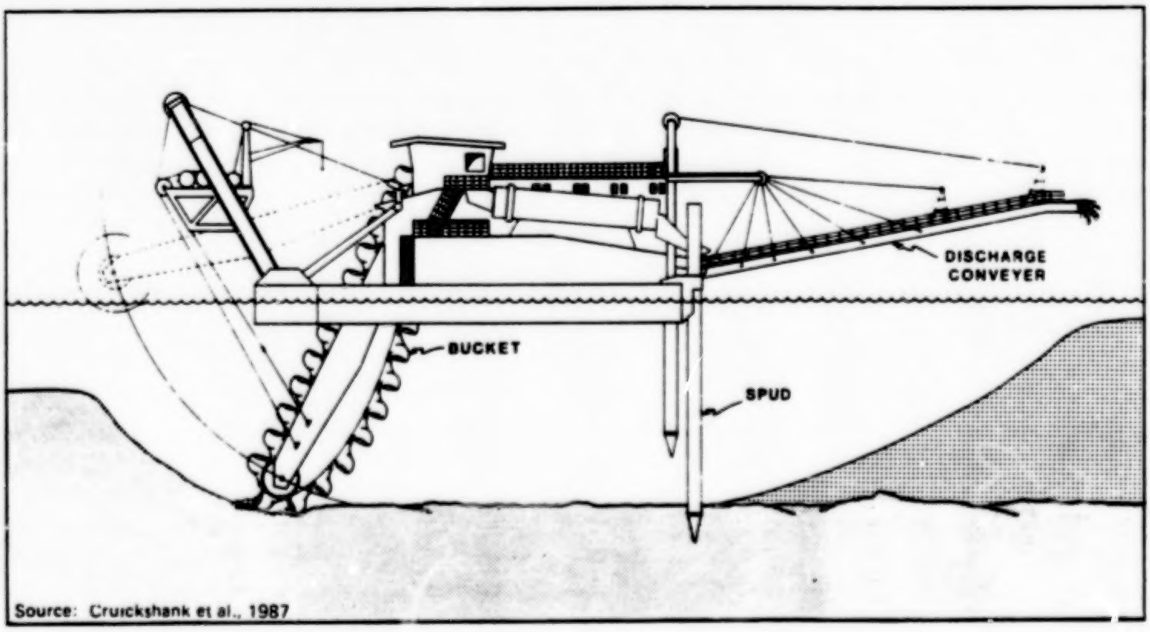


Figure 4-1. The BIMA – the world's largest mining vessel.

Once the material passes through the screen it goes into our radial jigs. What gets through those jigs goes into secondary jigs. These are just rectangular boxes that operate the same way as the radial jigs. What gets through there goes to our gold room. We're still on board the BIMA. We're not using any chemicals, we're just using seawater. We have pulled the heavy metals out, which is largely gold, and the remainder goes out the back end of the BIMA, into the ocean. The discharge consists of a 20 in. diameter pipe that's been welded onto the bottom of large pipes, and extend below the surface of the water. At present we are using an extension roughly 5 ft below the surface.



Source: Cruickshank et al., 1987

Figure 4-2. Bucket ladder dredge.

Let's look into habitat alterations. What type of habitats do we have? It's largely fine sand, some medium sand. There is a lot of animal life in the sediments but it is rather boring after the first few minutes. We do get into cobble and boulder. The boulder communities are composed of sponges, crustose corallines, tunicates, and hydroids. There are also sea urchins, barnacles, and gravel, cobble, and fine sand in between boulders. Over most of the material there is a layer of silty material. Some boulders are quite large.

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Ray Emerson: Is that consolidated material?

Paul Rusanowski: This is undisturbed.

Ray Emerson: Right, but is that a conglomerate type, asphalt-type surface, or are we looking at loose particulates that are the size that lends themselves to the cobble classification.

Paul Rusanowski: If you were to disturb this surface you would be able to move these materials with some ease. These larger materials seem to be "cemented in." It requires some considerable effort to bust one loose. And once you get one loose, the whole surface moves and can be dug out with ease. It's getting the first one out that's the problem.

Ray Emerson: Okay, so those are features, though, that are a kind of a conglomerate sediment that need to be broken down when they're picked up by the dredge?

Paul Rusanowski: Right.

Ray Emerson: And so there's really two kinds of cobble, is what I'm getting at. I guess we've got the impression that there's loose material that is a larger substance that is moveable, but then there's also these kind of compacted sediments that are really a result of glacial induration probably, or something like that.

Paul Rusanowski: Most of what we have is the glacial material. We get very little of the other. It does occur out there, but we have spent very little time in it. There's very low gold values in the material that's not compacted.

Ray Emerson: Yes, the difference being that the loose stuff is going to settle, it's going to be the same. The glacially indurated material is not going to be restored unless we have another ice age.

Paul Rusanowski: I'm not sure that we're changing the actual composition of it that much. We have re-dug the tailing two years in a row. And on re-digging the tailings we find them almost as difficult to dig as the first time we dug them.

Ray Emerson: What would cause them to be consolidated?

Paul Rusanowski: We're not sure at this time. That was a surprise to us. We expected the tailings to be very easy to dig. It was very much of a surprise to us when we got into them, and getting into some more of our dredge courses that were virtually as tough to dig the second time through. And they had only been in place for one to two years. So whatever is going on, they're obviously settling very fast, even though the fines within them have been redistributed.

Ray Emerson: So smaller particles are actually cementing themselves back together?

Paul Rusanowski: Don't forget our discharge is all coming in the same area. The oversize and the fines are all being discharged in the same place. So as the large particles wash through the system, they're getting dumped into these fines, which are being piled behind us. So it may be a matter that we're optimally distributing the particle sizes in the tailings, rather than what may naturally have occurred as a result of glaciation. We may be producing a better environment for setting up for these sediments, isolating the larger boulders with a considerable amount of sands between them. We don't quite know the reason the consolidation —.

Ray Emerson: I guess I'm confused on why the sediments would reconsolidate after requiring glacial pressure initially.

Paul Rusanowski: The surface is not consolidated, however, the difficulty of digging is the same in the tailings as it in the original material. That is what the surprising aspect is, and we would not have expected that.

Our whole entire lease block area has been mapped with side scan sonar to produce maps of the distribution of all the sediments surficially within our mining area. We have several general categories: cobble patches, sand patches, sand waves, and sand and gravel mixtures.

Table 4-1. Expected turbidity levels.

Discharge (MGD)	Depth (m)	Silt Content	Range of Turbidity Levels (NTU) 500 m	600 m	700 m	800 m
35	10	0.5-15%	0- 33	0- 53	0-37	0-23
35	18	0.5-15%	0- 90	0- 65	0-46	0-32
35	22	0.5-15%	0-104	0- 80	0-60	0-44
50	10	0.5-15%	0- 15	0- 9	0- 6	0- 2
50	18	0.5-15%	0-116	0- 85	0-62	0-44
50	22	0.5-15%	0-134	0- 98	0-72	0-52
65	10	0.5-15%	0- 29	0- 20	0-13	0- 9
65	18	0.5-15%	0-154	0- 95	0-69	0-48
65	22	0.5-15%	0-162	0-118	0-88	0-62

Using models developed by EHI in Redmond, Washington, we're able to predict the range of turbidity levels that we expect under different discharge conditions, water depths, and silt content of the material that we're digging (Table 4-1). In this case we have to be looking at 500 to 800 m away, these are values above 25 nephelometric turbidity units (NTUs) higher than the background. So the model starts with the background, adds 25 NTUs to it, and then runs the prediction curves to find out what would happen. For example, under the first set of conditions, at 800 m, you would expect operating conditions to be between zero and 23 NTUs above background. In this case, you would be within the 25 NTU envelope that we're looking at in terms of water quality for the region. In the next case down we have gone to 18 m instead of 10 m. You can see that our range would then be zero to 32 NTU.

The silt content has typically been 15% or less in all of our materials. We have gotten silt contents up to 40%. This is silt range particles, not in an amorphous silt classification, but this is a particle size classification.

Discharge volume reflects 35 MGD is for average operating conditions, 50 MGD is high, and 65 is above what we're presently capable of producing in our discharge.

This gives you an idea of what to expect in the water column. If we were to extrapolate this out further (to a 1000 m), we see that in excess of 95% of the time, we're in conditions of turbidity less than 25 NTUs above background. Not 100% of the time, but approaching 95% of

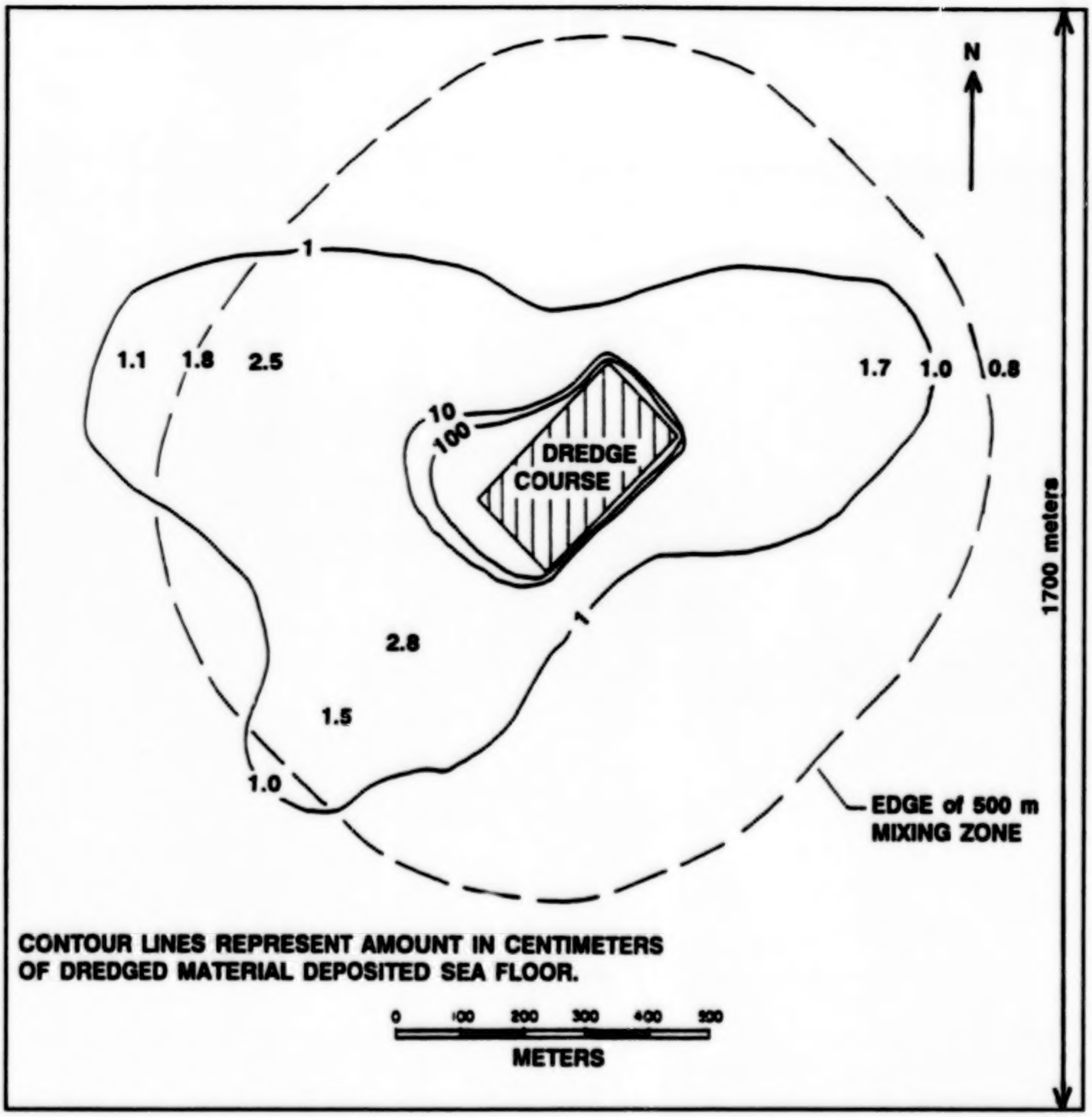

N
1
1.1
1.8
2.5
1.7
1.0
0.8
10
100
DREDGE COURSE
1700 meters
1
2.8
1.5
1.0
EDGE of 500 m MIXING ZONE
CONTOUR LINES REPRESENT AMOUNT IN CENTIMETERS OF DREDGED MATERIAL DEPOSITED SEA FLOOR.
0
100
200
300
400
500
METERS

Figure 4-3. Accumulated volume of solids distributed over a 1700 m by 1700 m area for a 20-day dredging period as estimated by the DIFCD model. The model assumed a dredge course size of 240 m by 130 m, a water depth of 12.1 m, a current velocity of 0.39 kn, a silt content of 15% in the dredged material, and a digging depth of 4 m. The model also assumed deposited material would not be transported away by waves or currents.

the time at 1000 m. So if we are looking at water quality effects, habitat alteration if you will, of the water column, we're primarily looking at that 1000 "window".

We can use the same modeling to predict deposition. In this case we have a dredge course, and we have dredged 100% of the material. Figure 4-3 represents the pattern of deposition for 100 cm, 10 cm, and 1 cm of deposition above the natural sea floor. It incorporates the current speed and direction that we would see during this kind of operation. As can be seen this case, the dotted line represents 500 m outside of this dredge course. Almost all of the sea floor deposition is within 500 m of the dredge, even though we have turbidity in excess of 25 NTUs above background extending out to 1000 m. Obviously if we stacked dredge courses next to each other, and lengthened the time we could change this pattern somewhat, because this is idealized to one single dredge course.

If we are looking at burial problems, or interaction with fine particulate sediments with the organisms, it covers an area of about 135 acres.

____: That's in one day?

Paul Rusanowski: No, this would be maybe in a month. It's going to depend on the depth of excavation. That has been optimized in this particular case to an average digging depth of 4 m, which in shallow water represents the average depth that we've been digging. If we dig at 1 m or 2 m it would be different, or if we dug at 10 m it would be different.

I pointed out eariler that site R-6, (the 1986 dredge area) was the one we were concerned with most. Figure 4-4 illustrates the bottom topography at the site since dredging. This is one year, two years, and three years after dredging. The visual effect that you see is that this has high relief features, lots of hills, lots of valleys.

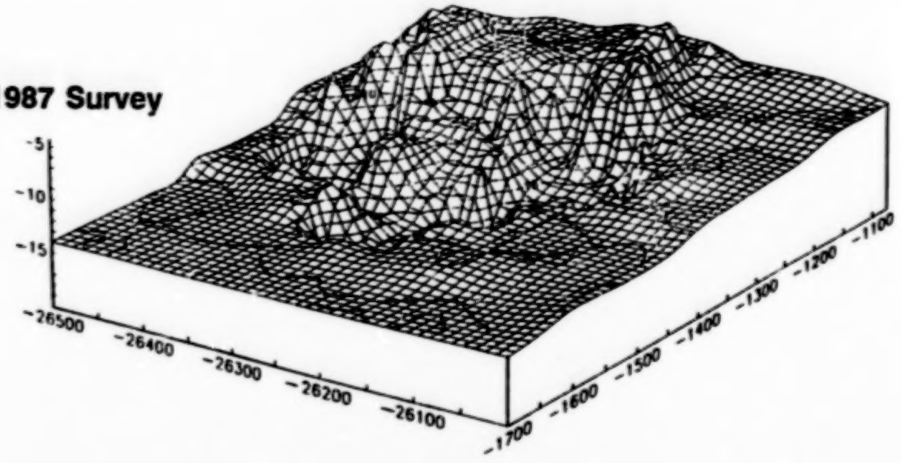
If we look at the changes over time (Figure 4-5), it represents roughly a 1 m change in elevation per year. Much, much slower than we had anticipated to occur. What we're seeing is that the valleys are filling in, and the hills are eroding. If we look at it in a different fashion, and we start layering the information, this diagram represents bathymetry and sediment types over the footprint. As you can see, much of the footprint is changing. This indicates that we are measuring changes of more than 1 m in elevation between successive years. We have matched the sediment within that area, with solid lines being an increase, and dotted lines being a decrease in elevation of more than 1 m. We have looked at the substrates in each area and we see change (Table 4-2). If we look at the valleys, we're seeing more sands than cobbles or gravels. And if we look at the hills we're seeing more cobbles and gravels than sands. This may indicate that when the area does stabilize we're going to end up with mounds of cobble and valleys that are filled in with sands. But this is the type of thing we're doing with our computer manipulation of the databases.

If we look at composition changes in the control area from 1985 to 1988 (Table 4-3), these four categories are not stable across time, they change. The natural environment is changing. If we convert this to our lease wide area, within our 30 ft zone, then we would see roughly a 5% areal change per year in substrate types.

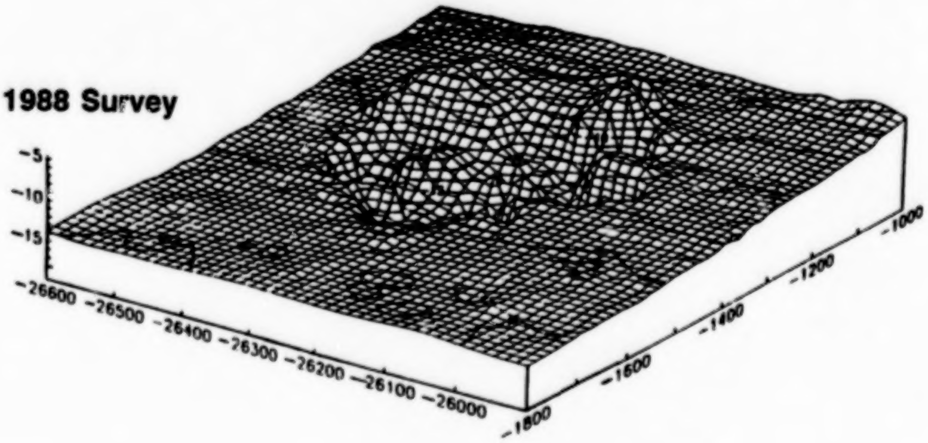
This is an important feature to remember when you're looking at the benthic environment, because naturally we're seeing the areas change by 5%. So if you happen to go out and sample at a location, you've got to be very sure that that substrate has stayed the same for that period of time. And it may not have, this type of data indicates we're seeing considerable change in the natural areas.

Woollcott Smith: Okay. So there are a lot of points in your side scan?

October 1987 Survey



October 1988 Survey



September 1989 Survey

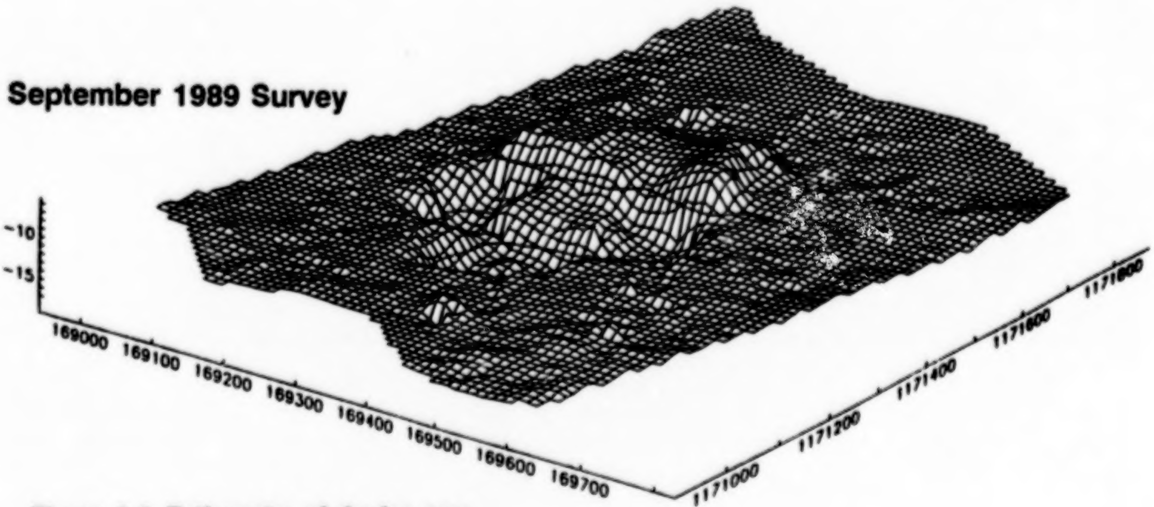


Figure 4-4. Bathymetry of dredge area.

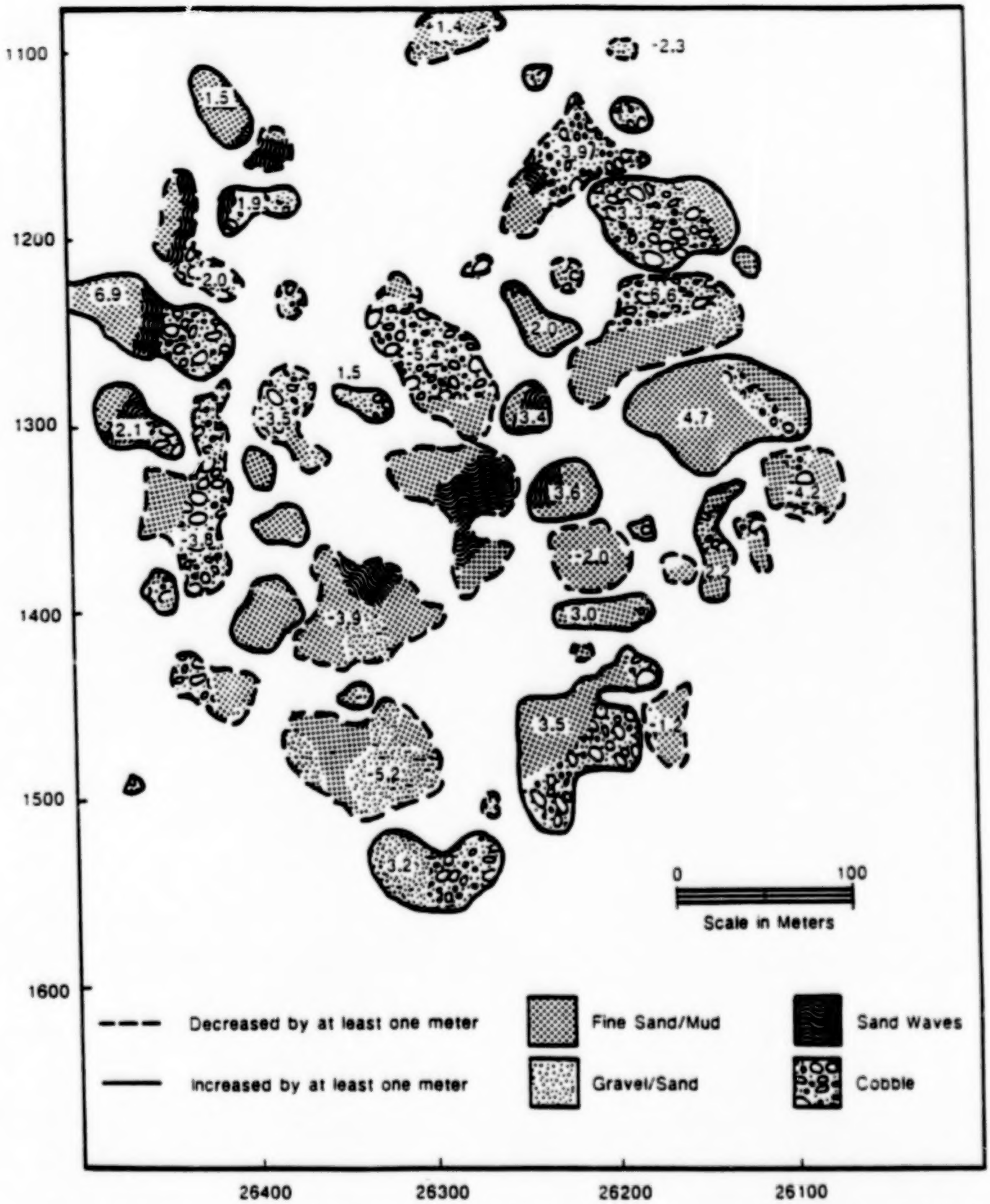


Figure 4-5. Two-dimensional representation of bathymetry changes to the 1986 dredge area between October 1987 and October 1988. Map shows substrate types for areas which changed at least 1 m in height or depth.

Table 4-2. Seafloor type by percent composition in mined areas.

	R-6			R-7	
Substrate	Before Mining	1 Year After Mining	2 Years After Mining	Before Mining	1 Year After Mining
Sand/Silt	41	70	42	20	77
Sand/Gravel	44	30	11	23	18
Cobble	11	0	42	46	3
Sand Waves	4	0	5	4	2

Paul Rusanowski: Yes. We started looking at mined versus other areas. For R-6, we have data before mining...roughly half sand, half sand gravel, a little bit of cobble, and a small amount of sand waves. We see the changes that occur within two years. I don't have the information on the third year here today, but the numbers are somewhat similar to these, although they have changed.

Table 4-3. Seafloor type by percent composition in control area, 1985-1988.

Seafloor Sediment Type	1985	1985	1988
Sand/Silt	37.8	37.3	45.4
Sand/Fine Gravel	0.0	0.0	0.0
Cobble	58.2	62.0	53.9
Sand Wave	3.9	0.7	7.0

R-7 is a larger area, same relative depth. Again, before mining we're seeing cobble, and then sand, and gravel, and sandsilt areas being approximately equal to the cobble area. This area was a very dominant cobble area except for one corner. And we see the change. We lose the cobble in the first year, but as we go with time we get back to the same kinds of substrates as we started with, but a different relief. So we're seeing a relief effect, not a substrate change.

We look into the biological system. Here we see one way of presenting the data. The pre-mining baseline is all of our information that we have where no mining activity occurred. We pooled it and generated average numbers. For the post-mining baseline we've taken the same information for that year, and done the same thing. So now we're looking at basically three years worth of data. So those data correspond to two years after mining, but it's the same stations as presented here before mining occurred.

So you get a feel for it by just looking at those stations. We had 62 kinds of animals before we mined, and now we have 72. So you can see that in your baseline, things are changing. This is comprised of multiple stations, multiple depths, multiple strata. So you can't fix on any single parameter.

The important thing to note is you have got to look at lots of parameters to make any sense out of it. We start looking at the data between years, and we can see a lot of patterns that developed very quickly. The one that stands out best is the biomass per m^2 (Table 4-4). The mining areas have very low biomass per square meter after mining. Of the other parameters, the only one that stands out is this biomass. All the rest are lost within the noise. It is a difficult situation to analyze.

Table 4-4. Results from 1988 benthic sampling program, Nome, Alaska.

	Sample Stations				Mined	
	Control				R6	R7
Substrate	C2	S2	C3	S3	2 Years After Mining	1 Year After Mining
Sand						
Kinds of Animals	24	21	28	52	23	14
Number of Individuals/m^2	2452	1278	1040	5127	1308	367
Biomass per m^2 (grams)	93.9	4.9	10.6	51.2	7.3	0.3
Cobble						
Kinds of Animals	32	20	25	39	15	14
Number of Individuals/m^2	1053	738	1035	2495	597	370
Biomass per m^2 (grams)	35.5	12.8	26.5	17.0	0.7	0.6
Motile Epifauna - Cobble						
Kinds of Animals	4	5	5	8	6	6
Number of individuals/m^2	278	295	1343	972	1452	1062
Biomass per m^2 (grams)	1.9	0.1	0.1	0.1	0.2	0.11

For motile and non-mobile animals, we thought there was some good information here, but it's turned out to be a bust. We see an obvious shift (Table 4-5), and it comes back to "half and half." We are not sure what's going on. So we are abandoning our approach on motility.

Gail Irvine: Would you explain again what the significance of the post-mining baseline is, I mean, how that relates to the other years? I didn't understand that.

Table 4-5. Biological changes in sand substrates.

Category	Pre-mining Baseline	1 Year After Mining	2 Years After Mining	Post-mining Baseline
Kinds of Animals	62	24	58	72
Kinds of Animals/Replicate	32	9	23	37
Number of Individuals/m^2	1765	167	1308	2411
Biomass/m^2 (grams)	70.8	0.9	7.3	35.5
Average Weight of Individuals (grams)	0.039	0.005	0.006	0.016
Percent Motile Animals	33	64	54	57
Percent Non-motile Animals	67	36	46	43

Paul Rusanowski: This post-mining baseline is the same stations that were used here. But the data that was used to calculate these numbers came from this area two years after mining. So we looked at this area in 1986, and these are the same stations in 1988, just two years later. That's all we've done and we've ignored everything else in between.

We can look at it in a different way. Here again we see mine stations and our sample stations or controls. This is sand we're looking at, we see the range in the kinds of animals, the number of individuals, and the biomass. Again, the biomass is the key one that comes out. We look at the cobble substrates. We see different types of biomass numbers, but that one seems to be the big one that stands out.

The important thing to note here is that, in looking at cobbles, we don't see the same pattern as in sand. So if we're looking at soft substrate recovery, or recolonization, within a year after disturbance we're seeing animals coming in. Within two years we're approaching normal ranges on most parameters. Within three years we're within normal ranges on all parameters, including biomass. But biomass is still very low. We look at cobble, and we're at least one year out of sync. We don't see recovery on cobble until two years after disturbance. Three years after disturbance we're seeing increased numbers. So the cobble community recovers at a slower rate than the sand community.

Ray Emerson: Do you think the composition of the communities in this cobble substrate are similar before and after?

Paul Rusanowski: No, they're not. We're dealing with intermediate stages, different animals, different percentages. And we're seeing changes in the dominant species between years. Even though our kinds of animals are coming out the same, the number of individuals, our biomass is coming up. The species assemblage that makes them up, the names of the animals are changing.

Ray Emerson: Okay.

Paul Rusanowski: So that's still going on. So even though the numbers look good, we're still at intermediate stages, not in a recovered or a stable community. We're in an aggressive recovery phase where the animals are jostling for position. In year three, we see a tremendous influx of clams. We don't see clams in our natural population in very large numbers. So we expect to see a big shift next year in our data, because all these clams that settled are going to be dead. They just aren't going to be there, because we never see them in our natural system. So they're obviously taking advantage of a situation that's good for settling. But they aren't going to be there, so we're still going to see lots of changes going on.

But you've got to remember that all these different kinds of parameters, on balance, aren't telling us very much.

Ray Emerson: Well, we would expect to see some kind of normal successional sequence there that would resemble the previous community structure if indeed the substrate type were similar. I guess I'm getting back somewhat to the question of two kinds of cobble that we never really distinguished, saying that it's the same when there's certainly an unconsolidated cobble that's left. There's also, you're saying now, a consolidated cobble that's being produced somehow from the previous form. I would say that isn't that a noteworthy difference if you're going to look at community succession? Or do you think that all the cobble that's been decomposed, or broken up into fine particles, is going to cement itself down again and become a very stable substrate, because that certainly affects the type of community that's going to be there?

Paul Rusanowski: My guess right now is it's going to cement itself down into a stable substrate. Don't forget all these numbers we're looking at after mining, we're dealing with a substrate relief that's changing by a meter a year. Now we can't pick a specific individual location, but we know that more than half of the substrate available for us to monitor is changing by a meter a year in elevation. So you're dealing with animals that must be capable of withstanding dramatic changes in the amount of substrate that exists, either by continuing to bury themselves deeper or digging themselves out of whatever is happening in that location.

So we're in a real dynamic response situation by these organisms. They're, in my opinion, very well adapted to change. That's the mode of lifestyle that they're in. Everything is changing. So they're used to moving around constantly. So the fact that we have an unstable substrate isn't affecting their ability to take advantage of this space that's been created by the dredging activity. But we're not in the stable long term community that we started with by any means.

Ray Emerson: Do we have any other references from dredging activities in this type of a consolidated substrate that indeed it does become reconsolidated, after dredging activity? I think that's a pretty important difference here.

Paul Rusanowski: Not that I know of, because we don't have any other specific mining activity that has dealt with glacial material.

Ray Emerson: I wonder is there any speculation, Sathy, as to why sediments would re-cement with pressure?

Sathy Naidu: Please rephrase your question.

Ray Emerson: I guess I'm asking, even without the community present, would you expect glacially indurated sediments, once they're broken up in the marine environment, to re-cement and consolidate, to form a relatively hard surface?

Sathy Naidu: Probably, should they be given sufficient time.

Ray Emerson: Yes, but then when it's broken up by dredging it becomes fine material, it's broken up in its respective particle sizes, now it settles out on the bottom, now suddenly it becomes hard again to - I don't know, it sounds to me like Elmer's glue is in there.

Sathy Naidu: But are they getting hard there?

Ray Emerson: Yes, yes —.

Paul Rusanowski: Don't forget we're talking of the "digability."

Sathy Naidu: Yes. As hard as in the normal glacial sediment, or —.

Ray Emerson: Is that what you're saying, Paul?

Paul Rusanowski: Yes, but we don't know what the surface looks like. All we know is that the "digability" by the dredge is virtually the same whether we're digging in tailings, or whether we're digging in virgin material. That's the surprising aspect. So we don't know what's going on within the tailings, but we don't know what the surface of those tailings are in terms of stability. The pictures we have from our areas indicate that the surface layer is very loose and unstable. There's no problem in moving the cobbles and rocks, they're all very loose.

Ray Emerson: Okay, so then we should talk about resettlement, recolonization, in terms of the substrate type here that is consolidated cobble, and unconsolidated, or - there are two definite, I think, distinctions that need to be made. What would you say, Sathy?

Sathy Naidu: I agree with you, distinctions must be made as to the possible two types mentioned by you.

Gail Irvine: I have one other point I'd like to make. This is a small aside basically. There is kind of an assumption that when you disturb an area, and then you have animals colonizing or moving into it, that you're going to follow the predictable sequence of succession. But that's not always the case, it depends on the actual mechanisms of interaction among the species. So in some cases it's documented in succession that a colonizing species will come in and preempt species that used to be abundant.

So that's something to be aware of, and it's an important aspect of looking at which species actually are found before and during the process. And then through time anyway, through the whole successional sequence, because in fact, even if the things look aggressive, you may not be ending up with similar species composition.

Jerry Kudenov: This is in fact what I think is going on. Typically, species that are tube dwellers, very small, much smaller than what originally occurred in any sort of habitat, will recolonize a perturbation area. Such as one perturbed by dredging, or some natural perturbation. So what I see here in terms of these data is a trend, especially in terms of biomass, where smaller individuals have replaced what was originally there to begin with. And the process is still going on, just essentially it is a typical recolonization pattern.

And it's not too surprising to me that within a year or so, we're seeing these sorts of animals recolonizing the tailings, the dredging spoils. What we're going to see are those opportunistic species that can exploit that type of very unstable habitat.

Paul Rusanowski: Okay, moving on to the next aspect, since we've been talking a lot about mercury. And since I've got the floor I get to present a little more data, throw my spear into the ox that's running around the room.

Let's talk about seals. What I've done here is put together some information, taking the data we've monitored in Norton Sound and comparing it to other locations, either in Alaska or Canada or somewhere else in the world. We looked at bearded seal. I've just looked at the meat and the liver in this case. We're looking at very few animals. We only have eight or nine seals total in our population, so these numbers here may represent only one animal. But the number we have for bearded seal meat is 0.03 ppm and in the liver 2.28 ppm. In the literature we're able to find bearded seal at other locations in Alaska, at 0.2 ppm in the meat, and 1.91 ppm in the liver. And then if we look at them in Canada, we see again the meat 0.35 ppm, which isn't bad, but the liver is 143 ppm. So we're seeing considerable variation.

If we look at the spotted seal, another subsistence resource in the area, again we see the meat at 0.11 ppm. The harbor seal, which is closely related to the spotted seal, is 0.57 ppm. Looking at the liver again, 2.26 ppm in Norton Sound, and 4 ppm and 8 ppm in Alaska and other Canadian locations.

The ringed seal data shows the same type of pattern, we see 0.01 ppm in the meat, 0.45 ppm and 1.62 ppm in other locations. And again, in this case 0.74 ppm in the liver, but 13 ppm and 86 ppm in other locations. You see the general pattern that, in the marine mammals in

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Norton Sound that we've measured, again only 8 or 9 animals, they're all consistently on the low end of the ranges that are being reported in the literature.

If we look at some of the other animals, and a lot of people have been drubbing on EPA, but here they're coming to our rescue. The first column is data that was generated by the EPA for Puget Sound on various kinds of animals. This is the data we've generated for the species we've been monitoring. And then these are values from other sources in the literature that we've looked at, for various animals.

Clams, we're seeing 0.023 ppm in Puget Sound, 0.02 ppm in Norton Sound, and literature values of 0.025 ppm to 0.06 ppm, very consistent. Dungeness crab we're seeing 0.065 ppm, 0.052 ppm, pretty good. Red king crab, our data averages 0.014 ppm, other literature values 0.092 ppm, so we're considerably lower than others. Salmon, here's your 0.053 ppm from Puget Sound, and other sources in literature 0.01 ppm. We don't measure it in Norton Sound. Arctic cisco, which is eaten, 0.035 ppm. Saffron cod, one of the cod that's taken through the ice, 0.019 ppm. And then here's some ranges for some other fish. So you can see from the Northwest and Alaska, these values are relatively low compared to a lot of the other information reported.

Sathy Naidu: Were the analyses reported in context of standard samples, NBS standards? How do I know that the numbers that you are giving me are accurate numbers?

Paul Rusanowski: Okay, all of our data is backed up with laboratory standards, independent standards, and what not. I haven't included any of that here, but we do have dogfish, and bovine liver, and a few others that are routinely run. Every time we send in a group of samples, we have standards that are run, plus the calibration curves and the standards on metal samples for their equipment calibration. So we know what their calibration levels are, the recovery levels, and then the reference samples for every time we send in a set of samples.

Table 4-6 shows the results of studies on mussel bioaccumulation. You can see from the shell length at the bottom, we did have growth. I'd like to just mention something briefly here, we're looking at baseline, that came from Shotgun Cove, Whittier, Prince William Sound. Mercury levels of 0.2, 3, 4, very, very high μg/g. We've not found anything like that in Norton Sound. We moved the animals into Norton Sound, and this is what happened to the mercury as described yesterday.

And you notice when you start looking at the other metals, they've all adjusted. These are the controlled animals, they were moved, and they were not in the turbidity plume of the BIMA.

We now have another set of data that carries us out for roughly a hundred days, and we see the same numbers. And we were able to add spat to the list, and the numbers again are the same as what we're seeing right here, very little change between them.

So where does that take us? I'd just like to say that, when we start looking at all of this monitoring work from an industrial point of view, we climb up on that marble pedestal the same as the academicians, and we have our crystal ball under our arm. We look into that, we throw in a little bit of scientific methodology, when it seems to support our case, and a little bit of scientific intuition to feed our egos, and we try and figure out where we're going.

Table 4-6. Mussel bioaccumulation.

	Baseline 7-14-89		Exposed to Turbidity Plume 69 days		Field Control	
Metal (µg/g)	$\overline{X}$	S.D.	$\overline{X}$	S.D.	$\overline{X}$	S.D.
Arsenic	0.96	0.168	1.17	0.125	1.15	0.169
Cadmium	0.614	0.084	1.68	0.125	1.72	0.464
Chromium	0.358	0.086	0.907	0.044	0.794	0.179
Copper	1.21	0.180	1.53	0.195	1.77	0.210
Lead	0.279	0.046	0.488	0.123	0.433	0.174
Nickel	0.281	0.038	0.84	0.221	0.567	0.069
Zinc	12.4	2.14	14.67	2.90	17.42	1.62
Mercury	0.234	0.004	<0.00800	0.0008	<0.0097	0.00026
Mean Shell Length 7-15-89	31.7 mm		33.8 mm		33.8 mm	
Mean Shell Length 9-24-99	-		35.4 mm		35.9 mm	

Note: 3 replicates; 7-12 animals per replicate.

So this is our best guess of where we're going. We're looking down that road trying to figure out what it looks like. Obviously it changes as we go along, but as of a month ago these are the types of conclusions we can make (Table 4-7). I've broken them into three areas. First, the physical environment. I think the thing to note here is that the area is unstable, it's changing over time. And we're looking at somewhere between a five and ten year time frame for this area to stabilize. Our best guess right now is 5 to 6 years, maybe 7, but a year from now we might be saying 7 to 9 years. Right now, looking at it we say 5 to 6 years looks pretty good. Whether it is or not, well, we'll reevaluate that later.

Second is water quality. Here we just aren't seeing anything outside of the turbidity effects. You note that I've said something about natural storm events. The natural variation in Norton Sound in storms runs up to about 90, 95 mg/l of sediment. At 100 m down current, in the plume, we have never in four years of operation reached 90 mg/l. That's never occurred. We got up over 80 once in 4 years of operation.

So the types of turbidity impacts you're seeing, while they may persist over time, are not outside the range of those turbidity conditions that the organisms would be exposed to for brief periods of time due to natural events.

Third, the biological system, I won't stress anything here. But it's pretty much straightforward. The thing to note is that we're seeing differential rates of recovery and recolonization between sand and cobble substrates. And we're looking at, again, that 5 to 10 year window of recovery. That's our best guess right now. We're obviously not there yet. Even though under most biological parameters we're seeing normal type communities. There is some distance to go to reach predisturbance conditions biologically.

Table 4-7. Conclusions after four years of study.

Physical Environment

- Mined areas have increased amounts of sand/silt habitat compared to before mining conditions.
- After two to three years substrate types occur in same proportion as surrounding areas.
- Relief features in dredged area change by approximately 1 m each year.
- Persistence of dredge foot print estimated at five to six years.

Water Quality

- No change in oxygen concentration in water column.
- No change in trace metal concentrations of eight priority metals.
- Turbidity plume within natural storm event levels within 100 m of discharge.

Biological Ecosystem

- Sand substrates show some recolonization within one year.
- Within three years biological assessment parameters are within normal range of variation observed in area; benthic community at intermediate level of recovery.
- Cobble substrates show signs of recolonization within two years.
- Community recovery based on existing database is estimated at less than five years for sand substrate and seven years for cobble substrates.
- No avoidance of mined areas by King Crab.
- No observed impact to subsistence King Crab fishery.
- No mercury or other trace metals being concentrated in King Crab, fish or King Crab food items.
- No evidence of bioaccumulation of mercury by mussels placed in turbidity plume.

HABITAT ALTERATION SESSION

RECOMMENDATIONS

HABITAT ALTERATION SESSION

SUMMARY OF RECOMMENDED STUDY ELEMENTS

PARAMETERS	METHODS
Sediment	
Size distribution	Grab, core 10 cm
TOC	
Metals	
Bottom topography and Composition	Side Scan Sonar
Water Column	
Temperature	CTD
Salinity	
Particulates	Sediment traps, transmissivity
Currents	Current meters
Biological	
Infauna	
Abundance	Grab, core 10 cm
Biomass	
Epibenthos	ROV, video transects

When

June - August

Coordinate with other studies
Water column studies - continuous
Sediment, biological parameters 1/yr

Where

Revisit 1973 and 1988 sites

Additional sites - selection based on Side Scan Sonar results - representation

3 areas considered - different processes?

Water column parameters - optimal array

Replication ?

HABITAT ALTERATION SESSION

REVIEW AND RECOMMENDATIONS

Robert Smith: I sat down with the Habitat Alteration group and somehow I got elected to draw on the board, and then I became the recorder, and now I'm becoming the speaker, so we'll make it short and sweet.

The first thing that we discussed was the state of knowledge, that the hydrodynamics and sediment dynamics of the area were not well known. Without knowing that, it is hard to know what to measure, where to measure it, and how to measure it. The main emphasis is to get more information on the system.

The sediment is the habitat that's obviously being directly impacted, and the best way to measure that impact is to measure it directly with sediment size distribution. After some debate we included total organic carbon (TOC), and the suite of metals that were presented by earlier sessions. Bottom topography and characterization should be conducted with side scan sonar and use grabs for the other parameters.

What is happening within the water column will determine the transport of the materials to other habitats. We need to know where the material is going, we can then look at those habitats to see if they have been altered. Temperature and salinity should be measured to trace water masses, and current meters installed to monitor speed and direction. Particulates should be examined. Sediment traps and the measurement of transmissivity would provide information.

The biological parameters that are recommended are abundance and biomass of the infauna. Cores or grabs may be used, sampling in different habitats may require different equipment. For the epibenthos, a video survey with remotely operated vehicles would be a suitable non-destructive method of sampling, rather than trawling (destroying a lot of animals in the process). I've done some analysis of that kind of data, and you can actually get quite a bit out of it.

That summarizes what we want to measure. The next question is - When do we want to measure it? Obviously, sampling has to be completed between the time the ice breaks up and when the storms begin later in the summer. Sampling should be coordinated with the other studies. The water column studies should be separate from the sediment studies. It is proposed that putting an array of meters, sediment traps, etc. be placed on the seafloor at optimal location. Sediment and the biologic parameters should be measured at least once during each season, and repeated annually until the dredging begins.

Where should we sample? There were reportedly stations sampled in 1973. While we are not certain of the location, they should probably be revisited. This would provide a measure of the long term variability. Short term variability information can be recovered from data at stations.

Rather than blanketing the area with a huge grid of sediment samples, it would be preferable to do a side scan sonar survey and produce a "general map" of the bottom topography and habitat types. This would allow the orderly selection of sampling sites to ensure adequate representation for different types of habitats. This would be the most efficient.

In locating the sampling sites, we considered the three areas that we have discussed earlier (Figure 4-6). The "trench area" is separate from "inside the trench area" and "outside the trench area." These are quite different bottom types, and there may be different processes in effect. We would want to make sure all three of those were covered.

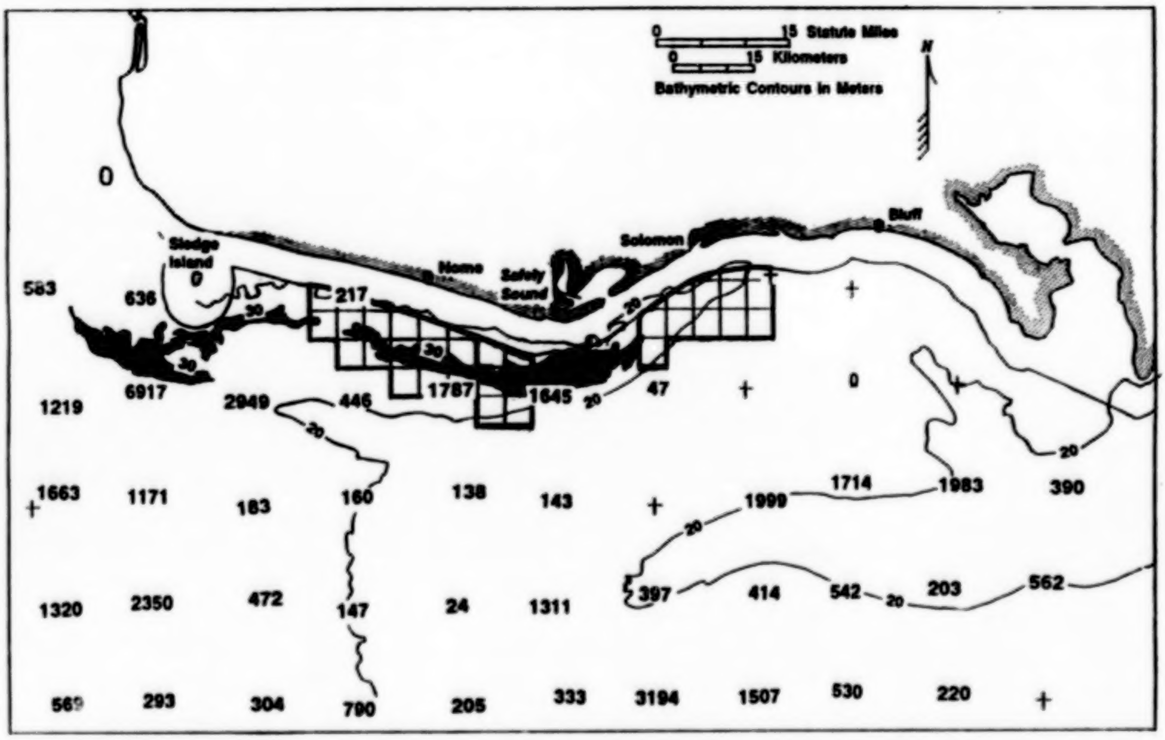

Figure 4-6. Bathymetry of Norton Sound. Dark areas indicate local depression. Numbers are estimated numbers (10s) of sublegal (<77 mm carapace length) tanner crab males per square nautical mile at stations sampled in Norton Sound in 1985.

The locations of the current meters and sediment traps, etc. have been left open. I am sure oceanographers will provide input.

We didn't have the opportunity to discuss replication. Are there any questions?

Woollcott Smith: Those three 1973 stations, are they on Eric's map?

Robert Smith: I don't know how many there are.

Eric Crecelius: In other words, we would like to match them up around three stations on there, east, west, and south, with the 1973 data, that would be desirable.

Robert Smith: So I guess nobody here knows exactly where the 1973 data were taken. Right, okay, so...

Sathy Naidu: I would like to add that, besides 1973, in 1988, we collected quite a few samples at about 12 stations. So we could use 1973 and 1988 stations.

Robert Smith: That's something that I guess all the groups have to consider to get a longer time series.

Chuck Mitchell: It seems to me, looking at the array that Eric had proposed that his array probably would be applicable in this situation that we were describing. It kind of fulfills some of the criteria that we were interested in. It appeared that some of the stations that were inshore, on the shelf area, if you can call it shelf, one on each of the transects was more or less in the trench, and then there was one offshore the trench. There might be a compromise, the actual locations could be modified slightly, but it looks like it would be a reasonable match.

Robert Smith: Yes, although if we had a side scan sonar survey, it could be better defined.

Chuck Mitchell: Oh yes, we would still want to have either additional stations or modify the distribution in some fashion, so that it reflected the different habitat types.

Ray Emerson: Have we covered data analysis, or is it addressed in a later session? My question relates to data analysis.

Robert Smith: I don't think anybody had time to get that far.

Ray Emerson: All right, then I'll ask it now.

Donald Hood: I'm afraid we don't have the data analysis section planned, maybe we should create one.

CHAPTER 5

HUMAN HEALTH SESSION

David O. Marsh, M.D., Session Chair
Tom Kosatsky, M.D., Assistant Session Chair

MONITORING OF THE NOME POPULATION

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Methylmercury (MeHg)

One year ago the human population studies on fetal exposure were discussed and a tentative conclusion was reached (Marsh 1989) that the lowest level for adverse fetal effect may be indicated by a maximum MeHg concentration in maternal hair during pregnancy of approximately 15 parts per million (ppm). No new data have been reported since that time, but a World Health Organization (WHO) task group has arrived at a similar conclusion i.e., that for fetal exposure, a 5% risk of effects in early childhood may be associated with a maximum maternal hair level of 10 to 20 ppm (WHO, in press). According to WHO (1976), a hair concentration of 50 ppm results from long term intake of 200 microgram (μg) of MeHg daily. It follows that the hair range of 10 to 20 ppm would be attained by long term daily consumption of 40 to 80 grams (g) of fish with average MeHg concentration of 1.0 ppm, or 80 to 160 g (2.6 to 5.6 oz) of fish with MeHg of 0.5 ppm. The suspected critical intake and hair levels for fetal effects may be reached or exceeded by Nome, or other nearby coastal residents through consumption of fish, shellfish, sea mammals or fish-eating birds. Any event that increases the MeHg concentration of these food would increase the body burden of MeHg.

The average concentration of MeHg in newly formed hair is 250 times greater than the simultaneous level in blood. Head hair grows at a mean rate of 1.1 cm per month so that a 10 cm segment, when analyzed segmentally, provides a calendar of fetal exposure throughout gestation. Seasonal variations in MeHg intake would be identified by segmental analysis of hair specimens 13.2 (5.2 inches) long. Newly formed hair takes about three weeks to grow to the scalp surface, so that the MeHg concentration at the scalp end of the sample represents the blood level three weeks earlier.

Maternal hair would provide an excellent index of fetal MeHg exposure. It is a tissue that is readily available without an invasive procedure. A pilot study might begin with hair sampling at maternity units within a day or two of delivery. Many samples would be long enough to recapitulate the entire period of gestation, and longer ones would profile the expected seasonal fluctuations in MeHg concentration. Any sampling program should be preceded by community explanation together with individual information and consent. A sample is obtained by isolating 30 to 40 adjacent strands of hair, gripping the bunch of strands firmly with forceps or between finger and thumb, cutting each strand as closely to the scalp as possible, tying the scalp end of the sample with thread and placing it in an envelope on which identifying data (name, age, date, address, unit number, etc.) are written. Hair analysis is usually by flameless atomic absorption spectroscopy. The laboratory should be experienced in mercury analyses, and employ internal and external quality control measures. The hair collection and analysis program might later be extended to include sampling at the first visit to antenatal clinics and nonpregnant women of fertile age.

Other Trace Elements

These included lead, arsenic, zinc, copper and nickel. Studies of the past two years suggest fetal lead effects at low threshold exposure levels indicated by maternal blood lead concentrations of 10 to 20 μg/deciliter (U.S. Dept. of Health and Human Services 1988). Arsenic, like MeHg, can be bioaccumulated in the aquatic food chain; most of the arsenic in fish is in the organic form, arsenobetaine. I am not aware of any studies of the effects of arsenic in fish-consuming populations. Clarkson, *et al.* (1988) have provided a recent review of monitoring these and other trace elements (Clarkson *et al.* 1988).

Toxic Trace Metals In Perspective

MeHg and lead are among the many factors that can influence fetal and early childhood development. The list includes family socioeconomic status, parental intelligence and education, birth weight, and maternal use of alcohol, tobacco and drugs of addiction (such as cocaine) during pregnancy. To the well established "fetal alcohol syndrome" and "fetal alcohol effects" has recently been added the first report (Little *et al.* 1989) that alcohol in breast milk may be associated with delay in infant development. The fetus or infant may be subjected to additive effects from several factors; a decrement of a few points on psychological testing may have only minor results for the individual but major impact on a community. It would be advantageous for any MeHg monitoring to be part of a larger program that gathers data on recognized covariables that can influence child development.

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PRESENTATION AND DISCUSSION

When I was here a year ago I talked only about mercury, especially methylmercury. I won't repeat all of that, but despite what we heard yesterday I don't think we should assume too early that mercury is no longer something to consider in the Nome area.

The large part of the mercury in the environment is of natural etiology. Mercury exists as a natural ore in the earth's surface. The big mercury mines in Almaden, Spain were exploited for 2,000 years. So a large part of the mercury in the environment, including the world's oceans, is of natural etiology.

Then, added to that are the results of human activities; such as smelting the mercury ore, the burning of fossil fuels, such as coal, and incineration of refuse including mercury batteries. All that gets into the atmosphere.

Whatever the form of mercury, whether it's elemental or inorganic compounds of mercury, or organic compounds like methylmercury, which is the form in fish; there is a global cycle of mercury. Any form of mercury can be biotransformed into other forms. So if elemental mercury from a broken thermometer escapes into the atmosphere, part of that could be biotransformed into methylmercury in the oceans.

In the oceans there's the food chain, or food web. Lower forms of life, like plankton, take up mercury. Little fish eat the plankton, bigger fish eat the little fish. We have this food chain with the big predatory fish like shark, tuna, swordfish at the top. Then we humans are at the top of the food chain if we eat the shark. This is biomagnification in the food chain.

Last year I discussed fetal methylmercury poisoning. I mentioned that the fetus is the most susceptible stage of life. The available data suggested that the threshold for the lowest fetal effect was a mother's hair mercury concentration of about 15 ppm.

Since that meeting here a year ago, a World Health Organization (WHO) working group met firstly, in Rochester, New York last December, and more recently in Italy. That international group decided "that the lowest effect level for the fetus may be, (this is the qualifier 'may be') indicated by maternal maximum hair concentration during pregnancy of 10 to 20 ppm." So it amounts to the same thing I said a year ago. There is no further data since that time but this is thought to be possibly the lowest effect level. It can be achieved by a relatively modest intake of fish.

The 1976 WHO document used the relationship that a long-term, steady, daily intake of methylmercury, of 200 μg/day will lead to a hair concentration of 50 ppm, equivalent to an average of 200 ppb in blood. The working group I mentioned put together a new WHO publication which is now in press. It will be a very up to date consensus document.

It will not be realistic to adopt the usual safety factor. Traditionally, when toxicological evidence tells us the lowest effect level for humans a safety factor of ten is added. If we have an effect level in the range of 10 to 20 ppm in hair, we can't then add to that a safety factor of ten. Fish eating countries have much higher levels than 1 to 2 ppm. So we can't add the traditional safety factor, and there's a question what a safety factor should be, maybe two?

Yesterday we heard about the hair analyses from 200 women in Nome. Now why do we analyze human hair? Well, the effect of methylmercury is on the central nervous system. The most vulnerable stage of life is the fetus. We can't really go around taking biopsies of fetal brains, so we have to find a substitute. We have to find something which is an index of the fetal brain concentration.

Yesterday, one of Dr. Scheuhammer's slides showed in birds, the relationship between the concentration in feathers, which are analogous to human hair and the concentration in the brain. We know that for methylmercury, the hair concentration is a good index of both blood mercury concentration and brain mercury concentration.

The basis for using hair analysis for many other elements has not been established. Hair is not a good indicator for elemental mercury. It may not be good for inorganic mercury, but it is excellent for methylmercury. Blood methylmercury is incorporated into the growing part of the hair follicle and is concentrated on average about 250 times higher in the hair than in the blood. It takes about three weeks to grow out to the surface. So when a hair sample is cut, the end closest to the scalp represents the relationship with blood about three weeks earlier. It is about 250 times higher than the blood level three weeks earlier.

The entire hair sample can be analyzed, but segmental analysis is better. We know that on average hair grows at the rate of 1.1 cm per month. So for pregnancy, if we have a 10 cm sample, and we chop the hair sample into 1.1 cm segments (or shorter segments) and analyze each segment, this provides a marvelous retrospective calendar of methylmercury exposure.

For example, in Nome, if there are people who still follow the traditional subsistence life style; hunting, fishing, and gathering; there will be some months of the year when their intake of fish, and seal, and sea mammals, and so on, provides a bigger dose of methylmercury than other months. So there will be a seasonal fluctuation. Hair samples, analyzed segmentally, would provide an annual profile of exposure.

Much of the basis for the WHO committee conclusion about the lowest effect level was derived from our study in Iraq. There was a huge outbreak of methylmercury poisoning in Iraq due to the use of a methylmercury fungicide. The seed grain had been sprayed with methylmercury as a fungicide to prevent rotting and disease. It was distributed free to farmers all over the country. They didn't need much of it for planting, so they had it ground into flour and baked bread in their homes every day. Some ingested a large dose of methylmercury.

We were not there at the beginning of this outbreak, but the following year (1972) we went around to the villages and remote farms and collected samples of mothers' hair and analyzed it segmentally. The mothers had hair several feet long. In this example, the hair was collected at the end of 1973. It was long enough so that when analyzed in segments it gave a calendar of past exposure going right back to before the outbreak that began in late 1971.

Where it says June 1971, that is the hair concentration prior to the period of consumption. Then you see the gradual increase of the hair concentration up to this extremely high level, some around 600 ppm. Then you see the decline, according to a half time of about 50 days. Parallel to that is the blood concentration. So this shows not only that hair provides a marvelous calendar of past exposure, it also indicates that it is closely related to the blood level. For each mother/infant pair it was possible to establish the period of pregnancy, the maximum exposure to the fetus, and the duration of exposure.

Yesterday, we were told about the hair analysis from 200 women of childbearing age of Nome. Last evening I was given the list of the 200, and whether each person was a Native or a non-Native. In Table 5-1, I have broken them down Native and non-Native. Roughly 90% of the total group were below 2 ppm. Four were slightly higher. Those were 4.0, 8.0, 4.5, and 6.2 ppm. This is a quite remarkably low level of hair mercury concentration for a population believed to regularly consume marine fish and sea mammals.

When I was here a year ago, I talked at some length with local residents, including people from Nome. They told me about the diet and the amount of seal that is consumed every week. I got the impression that large amounts of fish were consumed. The data presented in Table 5-1 would be the type of distribution you might expect to find in Rochester, New York, or Chicago, or Minneapolis, or any central North American city. It is not the profile that I would have expected in a society that consumes a lot of fish. Of course, the hair mercury concentration

Table 5-1. Methylmercury levels in hair samples of 200 Nome women of childbearing age.

Hair MeHg (ppm)	Number of Native	Number of Non-native
0-0.9	90	52
1-1.9	23	17
2-2.9	5	1
3-3.9	3	5
4+ (4;8)	2	2 (4.5;6.2)
Total	123 (61.5%)	77 (38.5%)
200 (22%) of 900 women		
Mean concentration = 0.97 ppm ± 1.03		

depends on two things: one, is the amount of fish or sea mammals consumed; and second, the concentration of methylmercury in that food. But the levels are much, much lower than I would have guessed. We have 200 hair samples from a target population of 900 women of child bearing age, which is 22% of the potential group. Now there are several possibilities. One possibility is that this is a representative sample; the analyses were all technically well done; and, that there is no problem.

There are several other questions. My colleague who will be speaking after me, is an epidemiologist, I hope he will talk a little about some of these. One is the question of selection.

Another question is that if there really is a subsistence, traditional life style, one would expect seasonal fluctuation. That could double the maximum concentrations. We need to find out whether there are people in Nome who really follow the traditional life style? I don't know the answer to these questions.

As I mentioned, one can get a lot more data from a hair sample by doing segmental analysis. This might also show that some of the maximum concentrations are higher than the ones indicated so far.

For these 200 hair samples, the mean methylmercury concentration was 0.97 ppm. As I mentioned earlier, according to the World Health Organization, the lowest effect level for adults is in the range of 50 to 125 ppm (I think the data points more towards the upper end of that range). The lowest effect level for the fetus is much below that; possibly 10 to 20 ppm. So the results we have seen from the 200 are well below that. If one accounts for seasonal fluctuation a few might approach 10 ppm.

What does that mean in terms of fish intake? As I said, for long-term intake, a daily intake of 200 μg of methylmercury results in a hair concentration of 50 ppm. Seventy percent of the 200 had hair levels below 1.0 ppm. So they would only need 4 μg per day to get a hair concentration of 1.0 ppm. This would be achieved by eating 4 g of fish per day at 1.0 ppm. Now 1.0 ppm would be the average concentration, for example, of swordfish. Canned tuna is about 0.33 ppm. So consuming 12 g/day of canned tuna would achieve that level.

Now this is far below what I understood: I thought Nome people were champion fish and sea mammal eaters. These data do not suggest that. Perhaps the consumed fish and sea mammals have unusually low average methylmercury concentrations.

Now I thought, at this point, I would say a couple of things about something I know little about. Yesterday, you remember the long list of elements and metals, everything from arsenic to cobalt. The reason that I don't think any of us here know much about them is that we have no reason to. They've never been listed as environmentally important for general populations. The group included arsenic, which, like methylmercury, does go through biomagnification in the food chain. So high levels of arsenic can occur in fish. But the chemical form of arsenic in fish is mostly the organic form of arsenobetaine. With mercury, the most toxic chemical form is the organic form, methylmercury. Arsenic is not like that. The organic form of arsenic is much less toxic than the inorganic, partly because of its very short half-time. The half-time of elimination for methylmercury is about 50 days, and for arsenobetaine it is just a few days, four or five days. So that really does not seem to be a practical hazard. At this point we really don't know if people in Nome eat appreciable amounts of fish. But if they ate large amounts it would be very difficult to see them getting any poisoning from organic arsenic in the fish.

Lead, nickel, and copper are present in the ocean water, but how do they get into people? You have to either eat them, or breathe them, or drink them. I don't know how that transition occurs from the water of the Sound to the people. But of those metals, lead is of relevance. If we think about the chemicals, or trace elements that affect fetal development, and early childhood development, methylmercury is one, lead is another, and maternal alcohol is the third. Those are far more important than any other group. You could add together all the ones mentioned yesterday, tin, and silver, and gold, and all those other things on the list, but none of them is important compared to methylmercury, lead, and alcohol.

There have been three very good studies in the last two years on the fetal effects of lead, one in Boston, one in Cincinnati, and one in Australia. They all reached the similar conclusion, that the lowest effect level of lead is a blood concentration of 10 to 20 μg/100 mℓ. The source of the lead is usually lead paint in deteriorated housing. Old houses used to be painted with paint that contained a lot of lead. That's no longer true. But older houses, especially with peeling paint, are one source. Leaded gasoline is another source. But how lead would get from the Sound water to people is not clear.

Nickel is a recognized industrial hazard in terms of carcinoma of the lung, and other pulmonary problems. I don't know of any environmental hazard to the general population. Copper is an essential element in many foods. There are one or two rather rare inherited metabolic disorders. Wilson's Disease is the most common, but it's still rare. It can cause neurological effects and cirrhosis of the liver. But I don't know of any environmental hazard to the general population from copper, or from any of those other metals that were on the lists yesterday.

Now I thought I would try to put things into a wider perspective. Recent data strongly indicate that methylmercury and lead are especially potent poisons to the fetus, but we have to keep some perspective. Many other factors have a negative impact on early childhood development.

If one considers the factors that can possibly have a bad effect on the developing infant, they include genetic, complications of pregnancy, labor and delivery; socioeconomic factors; parental education and intelligence; birth weight; gestational age; maternal age; and the maternal habits while pregnant. Alcohol is certainly very important, and I suspect is relevant in this part of the county. Smoking certainly has a bad effect on the infant. It's best to avoid prescription and over the counter drugs if possible during pregnancy. And certainly lead and methylmercury.

Now regarding alcohol in breast milk, there is a very interesting new article in the August 17, 1989 issue of the *New England Journal of Medicine*; which claimed that they were able to

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isolate fetal effect from breast milk. They also claimed to find a significant effect from alcohol in breast milk, apart from any fetal effect.

It is clear that many factors can have an adverse effect on the fetus. Even if one considers a population where the level of methylmercury in the maternal hair is 20 ppm, the fetal risk would be small compared with adverse fetal effects from maternal alcohol, smoking, socioeconomic and other factors.

HUMAN EXPOSURE AS A MONITOR OF ENVIRONMENTAL CONTAMINATION:
Its possibilities and limitations as illustrated by the case of methylmercury in Northern Quebec

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Given a test of exposure that is quantitative, reliable, and simple, it would seem relevant to sample humans as a monitor of the distribution of toxins in the environment. Experience with methylmercury in northern Quebec demonstrates the strengths and limitations. Methylmercury was identified during the early 1970s as an important contaminant of freshwater fish in northern Quebec; following extensive flooding for hydroelectric development during the 1980s concentrations rose in fish in connected lakes and rivers. Studies of exposure to methylmercury among the Cree Indians (for whom fish is an important subsistence food) during the 1970s and an ongoing monitoring program during the 1980s attempted to relate human uptake to the evolving contamination of the environment.

Data gathered since the mid-1970s show important differences in exposure of the northern Quebec Cree to methylmercury which relate to sex, age, village of residence, and activity as a trapper as well as large seasonal variation in exposure. No global relationship has been found between levels of methylmercury in fish available to Cree villagers and village-wide exposure to the toxin. For the most exposed group, however, trappers living in the woods, there is a suggestion of a close relationship between environmental contaminant levels and human uptake.

Negative publicity related to the presence of methylmercury seems clearly to affect exposure. So too does the ongoing monitoring program itself. Given our position in the food web, our ability to make dietary choices, and our capacity to be influenced by knowledge of environmental contamination, we humans are clearly of limited value as organisms to monitor environmental contamination. In contrast, human exposure monitoring is essential to the assessment of individual and collective risk.

This talk is somewhat iconoclastic, perhaps challenging people's images of what might be expected from an environmental monitoring program based on measurement of human health status.

I'm involved in monitoring exposure to methylmercury as consultant to the Cree Board of Health. The Cree are an Indian people living in northwestern Quebec, by the shores of the James and Hudson Bays (Figures 5-1 and 5-2). They've lived there for thousands of years, subsisting largely on a diet of freshwater fish, supplemented by the game and birds they hunt.

About 10 years ago the enormous hydroelectric potential of the subarctic forest home of the Cree was first exploited. A program was set up to tap that hydroelectric potential by the construction of large dams, which created big reservoirs. This greatly affected the economic and social existence of the Cree, as well as leading to the contamination of their fish with methylmercury through a process of leaching of inorganic mercury from the submerged rocks and its methylation by bacteria and plants associated with debris flooded in the reservoirs. I'd like to describe the way that a human health monitoring program, in place since the early 1980s, shows the effects of increased levels of methylmercury in fish following hydroelectric

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development on levels of methylmercury in people. Figure 5-2 shows the James Bay territory. Reservoir construction most affected the villages of Chisasibi and Wemindji. Fish taken by residents of other villages really has not been affected by hydroelectric development. I'll go back to Chisasibi and Wemindji later on when we try to compare levels of methylmercury over time and between villages.

Figure 5-3 shows some of the changes in levels of methylmercury in fish of the area. Northern Pike is a predatory fish and Lake Whitefish a non-predatory fish. Before development of hydroelectric reservoirs, methylmercury levels in flesh of the pike were about 0.7 ppm. In whitefish the levels were about 0.1 ppm. The reservoirs were constructed in 1979 and 1980, and by 1982 there was a large rise in the levels of methylmercury in both non-predatory fish and the fish which eat them; up to about 0.5 to 0.7 ppm in the non-predatory whitefish, and up to as much as 3 ppm in such predatory fish as the pike.

Again, the results for methylmercury in fish from lakes and rivers in other parts of the James Bay Territory remained at early 1980s levels, 0.1 ppm for non-predatory fish, and about 0.7 ppm for predatory fish. Rising levels of methylmercury only affected fish in the areas flooded for hydroelectric development.

Figure 5-4 gives you an idea of some of the results of our human health sampling program. The figure on the right refers to women between 15 and 39 years of age, and that on the left to men and women over 40 years of age.

Figure 5-1. Map of Canada.

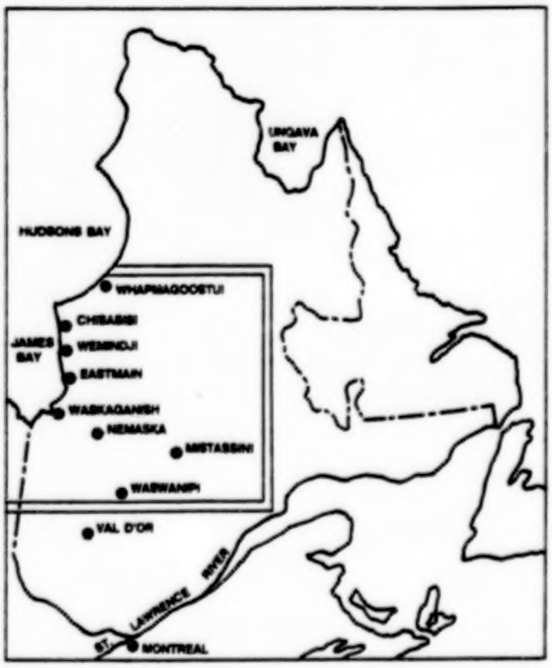

Figure 5-2. Map of James Bay territory.

We consider those two groups separately because they're really in two different risk categories. As Dr. Marsh indicated, for women of reproductive age the risk is to a developing fetus, while for older men and women the risk of neurologic disease is to themselves. Figure 5-4 shows the relative numbers of the population by degree of risk of disease related to methylmercury.

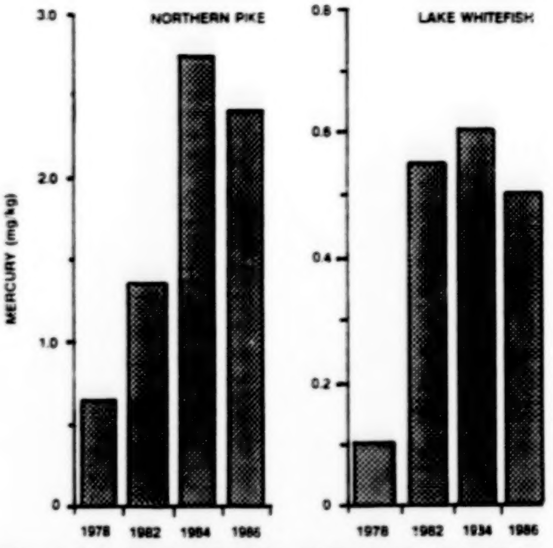

Figure 5-3. Concentration of mercury in fish in the LG-2 reservoir (From Evolution du mercure dans la chair du poisson, phase 1, 1986, Hydro-Quebeck).

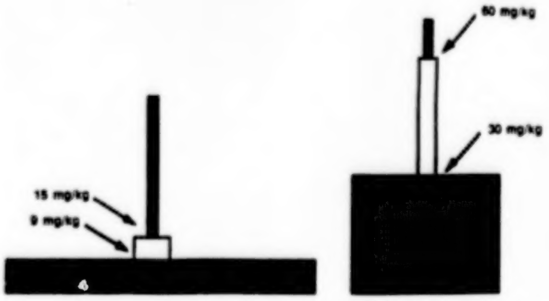

Figure 5-4. Mercury in hair, 1987. Persons above intervention levels.

If we look just at younger women, we can see that the vast majority, in fact about 92%, who make up the big black box, had a hair mercury analysis of less than 9 ppm. The transitional group between 9 and 15 ppm includes about 7%. There are about 1.5 to 2% who had more than 15 ppm of methylmercury in their hair. We consider this last group of women, were they to become pregnant, to have hair mercury levels representing a risk to a developing fetus.

For older members of the population, men and women 40 years and over, who are included in our sampling program, (and we sample about 80% of people in that group on a voluntary basis), about 93% had less than 30 ppm of methylmercury in hair. Only about 1% of people over 40 had hair containing over 60 ppm.

If, as Dr. Marsh said, the risk zone for damage to adults from methylmercury, begins above 50 to 100 ppm there is less than 1% of the population in that group. That's about where we were in both 1987 and 1988. So less than 2% of the population, both of younger women and of older adults are at risk of disease either to a fetus or to themselves through exposure to methylmercury.

We have looked for factors which determine likelihood of having a high methylmercury level. Among these factors are sex; males have slightly higher levels than females (Figure 5-5). Age is another important factor. Figure 5-6 shows all women sampled from 15 years to 70 + years across this horizontal axis; here is the median methylmercury level for each age group. Among older women, levels are very much higher than among younger women.

If we look at both the median (Figure 5-7) (the 50th percentile level indicated by the dark box), and also at the 90th percentile level, (the level below which 90% of the population falls – the shaded box) we can pick out an effect that's even clearer. When we look across age groups, this time among women 15 to 39, there really isn't much change from age group to age group, judging by the median. However in the most exposed group, those above the 90th percentile, there's really quite rapid change. Methylmercury level corresponding to the 90th percentile changes far more than does the median.

So sex is important, and age is important. Figure 5-8 shows that village is important. Here is the 50th percentile, and the 90th percentile value for each of nine villages. About 9000 Cree

Indians live in these villages. You can see that for the two villages where there is important methylmercury contamination of fish related to hydroelectric development – Chisasibi, and Wemindji, they're in the middle range. Other villages have slightly higher levels. And there are other villages, again, which have much lower levels.

Again, the levels of methylmercury in fish, other than in areas near to Chisasibi and Wemindji, would be quite similar. Fish from others villages would have levels of about a 0.1 ppm in non-predatory fish, and about 0.7 ppm in predatory fish, much lower than the Chisasibi and Wemindji levels.

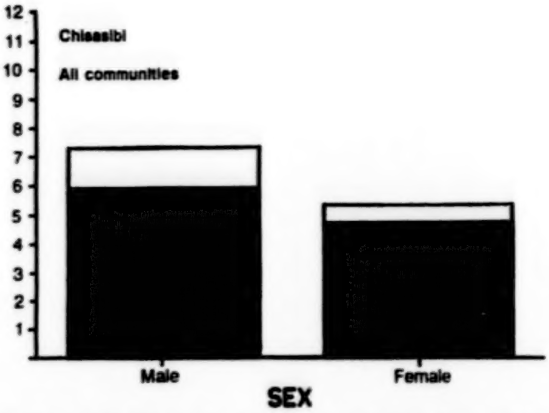

Figure 5-5. Mercury levels in women age 15 to 39 from six target communities, 50th and 90th percentiles, 1987.

So, age, sex, and village are all important. Since I'm an epidemiologist, I'd like to pick out those factors one from another. Here is one way of doing it, which puts all the data into a large mathematical equation, and separates out the individual effects of the various risk factors (Table 5-2). This technique, called logistic regression, was used to show the likelihood of having a value over 30 ppm, comparing one group to another. Under this first heading we've compared only the effect of age group – we've considered that the characteristics, sex, and village are held constant.

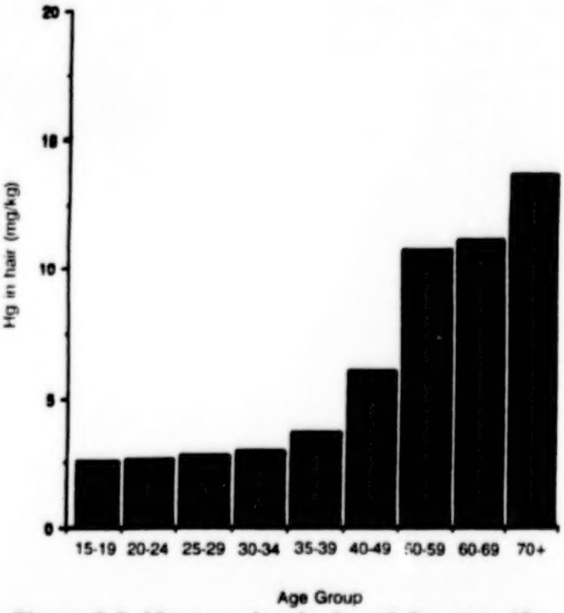

Figure 5-6. Mercury levels in adults age 40+, 1986.

When we look at the effect of age alone, we can see that among people 40 years of age and older, if we consider a risk level arbitrarily set at "one" for the age group 40 to 49, that from 50 to 59 there's a relative risk of 3.5 of having a methylmercury level greater than 30 ppm, of 3.7 for people in their 60s, 3.9 for people in their 70s. So there's really a big jump that occurs at about 50 years of age in terms of risk of having a high level. Again, that goes along with a previous graph, which demonstrated a big take-off at about 50 years of age in women.

Again, when you pick out sex alone as a risk factor, you can see that men are 1.6 times more likely to have a hair mercury level of 30 ppm or more than are women. When you look only at village: you can see that if Chisasibi, the village nearest the reservoirs is assigned a risk of one, then other villages, which are far from the reservoirs, have relatively higher risks. Oujebougoumou has a relative risk of 2.5. Another village, Waswanipi, 1.8; Great Whale River, again far from the reservoirs, is 2.5. Wemindji, the other village near to the reservoirs is 0.5. So

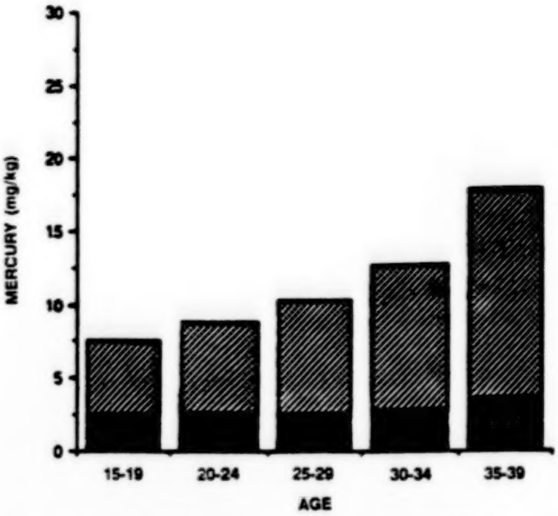

Figure 5-7. Median mercury in hair, 1986.

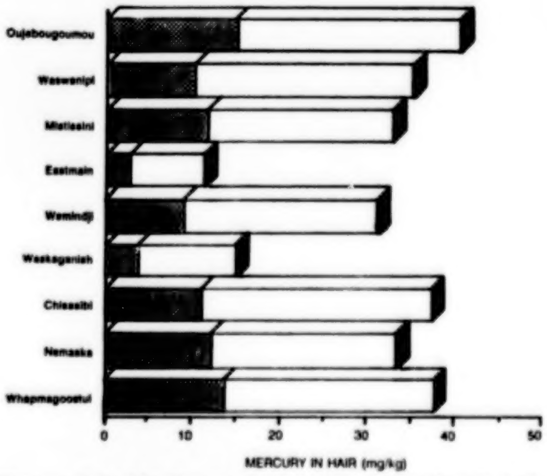

Figure 5-8. Median mercury concentration in hair in women, 1987.

when you control for age and sex, and look only at the effect of village, the villages that are in fact nearest to the reservoirs, nearest to the area with fish with high levels of methylmercury, are least likely to be associated with people having lots of methylmercury in them. How come?

Woollcott Smith: Could you tell us what both the total N and the number of individuals above 30.

Tom Kosatsky: The total N for that group is 1,000. And the number of individuals over 30 is 78.

Woollcott Smith: Yes. That is what is suspected.

Tom Kosatsky: Which gives you fairly large confidence intervals for some of the villages.

When we did the same analysis for 1988, we saw the same results. The pattern is the same. The estimate of risk is fairly constant from year to year. However our numbers are fairly small and in some cases we are not able to say that these results couldn't reasonably be explained by chance alone.

Woollcott Smith: Have you experimented with dropping the 30 ppm down to 15, or something like that?

Tom Kosatsky: Well, yes, and you find that these effects hold out. While the results enter the level of statistical significance, clinically a level of 15 ppm is unimportant. We're really only concerned with people who have levels over 30 or over 60.

Woollcott Smith: The question is, do you get the same relative risks?

Tom Kosatsky: You get very similar relative risks. The confidence intervals are much tighter, and show statistical significance at 1/20 for those villages that I mentioned. The other way we can show effects more clearly is to compare the high group and low group alone. Looking at people over 30 ppm as compared to people below 6 ppm, for instance, shows the same type of relationship with being a man, being older, and living in certain villages.

Beyond age, sex, and village, there's data that looks at the effect of whether or not people are trapping. I'll explain what I mean by trapping. There is an income maintenance program that went into effect in the early 1980s where-by people could opt for a life style that obligates them to live in the woods, live in the bush for four months or more a year. With it came, of course, a more traditional life-style than living in a village. These people are considered to be subsistence livers, people who are trapping as a major source of income. In fact, their income is supported given that they live in the bush for four months or more a year.

When we compare people who were trappers living in the woods for four months or more a year to people who were living in the woods for less than four months a year, i.e., who were not registered as subsistence trappers, we can see that being a trapper has a fairly important effect on methylmercury level (Figure 5-9).

Looking at this data we can see that trappers, both men and women, have higher median and higher 90th percentile methylmercury levels in their hair than do people who aren't trapping.

Consider men over 40 by themselves. In a logistic regression analyses which includes age, village, and status as a trapper, you can see that the relative odds of a trapper having a hair methylmercury level over 30 is 3.8 (Table 5-3). That level itself is not easily explainable by chance alone. So that's a fairly hard finding.

So we've seen among the James Bay Cree that village, age, sex, and being a trapper are important determinants of methylmercury level. Another important determinant of methylmercury level is season. Figure 5-10 is a graph from a paper written by Dr. Tom Clarkson, who is a colleague of Dr. Marsh. This is from a single person, an Indian guide in the Grassy Narrows Reserve in Ontario, who apparently had long hair. Combining serial cuts of hair, Dr. Clarkson was able to look at seasonal fluctuation over a four year period.

Table 5-2. The relative odds for having ≥30 ppm methylmercury in hair among men and women 40+ years resident in six villages sampled, summer 1987.

	Relative Odds[1]	Confidence Limits[2]	
Age			
40-49	1.0[3]		
50-59	3.5	1.7	7.5
60-69	3.7	1.7	8.2
70+	3.9	1.2	6.9
Village			
Chisasibi	1.0		
Oujebougoumou	2.5	0.9	7.1
Waswanipi	1.8	0.9	3.9
Mistassini	0.7	0.3	1.4
Wemindji	0.5	0.2	1.3
Whapmagoostui	2.5	1.1	5.6
Sex			
Female	1.0		
Male	1.6	0.9	2.6

[1] Most likely estimate of the relative odds (risk).

[2] 19 times in 20 the real relative odds are expected to fall between these two numbers.

[3] Chosen to be the reference effect.

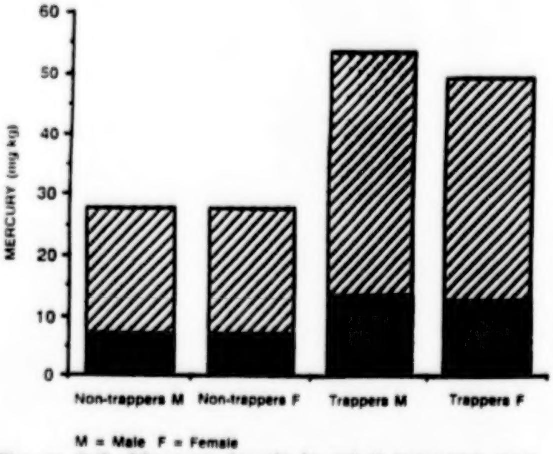

Figure 5-9. Mercury levels in adult trappers age 40+, 50th and 90th percentiles, 1987.

Well, if instead of looking at one person, let us look at the entire group of Cree with hair long enough to contribute 9 cm. In our population that leaves out almost all the men because

Table 5-3. The relative odds for having <30 ppm methylmercury in hair among men resident in five villages sampled, considering the relative effects of trapping, age, and village, summer 1987.

	Relative Odds[1]	Confidence Limits[2]	
Being a Trapper	3.8	1.6	9.2
Age			
40-49	1.0[3]		
50-59	3.1	0.8	12.4
60-69	4.4	1.1	16.7
70+	2.7	0.6	12.1
Village			
Chisasibi	1.0		
Oujebougoumou	1.0	0.2	5.2
Waswanipi	1.2	0.4	3.5
Wemindji	0.4	0.1	1.4
Whapmagoostui	1.5	0.5	4.6
Sex			
Female	1.0		
Male	1.6	0.9	2.6

1 Most likely estimate of the relative odds (risk).
2 19 times in 20 the real relative odds are expected to fall between these two numbers.
3 Chosen to be the reference effect.

their hair is almost always too short, and leaves in most of the women, especially the older women. We're able to see the effect of season. Figure 5-11 shows, looking backwards over serial segments of hair in women from 15 to 39 years of age, and in women 40 years of age and older, that there's a trough, a lowest point, that occurs during the winter.

Again, I'll show the transparency you viewed before that looks at methylmercury level by village of residence (Figure 5-8). There is quite a bit of variation among the different villages. The point that I made before is that there is not a clear relationship between methylmercury levels in fish from lakes and rivers near to the villages, and 50th or 90th percentile levels in people sampled in the villages. So that the villages that have most methylmercury in nearby fish are not the villages with highest levels in people.

However, if we look just at trappers, the people who live four months a year or more in the bush, we can make this correlation (Figure 5-12). In the village Wemindji, over the period of time displayed, from 1982 to 1985, levels of methylmercury rose in the nearby reservoir and

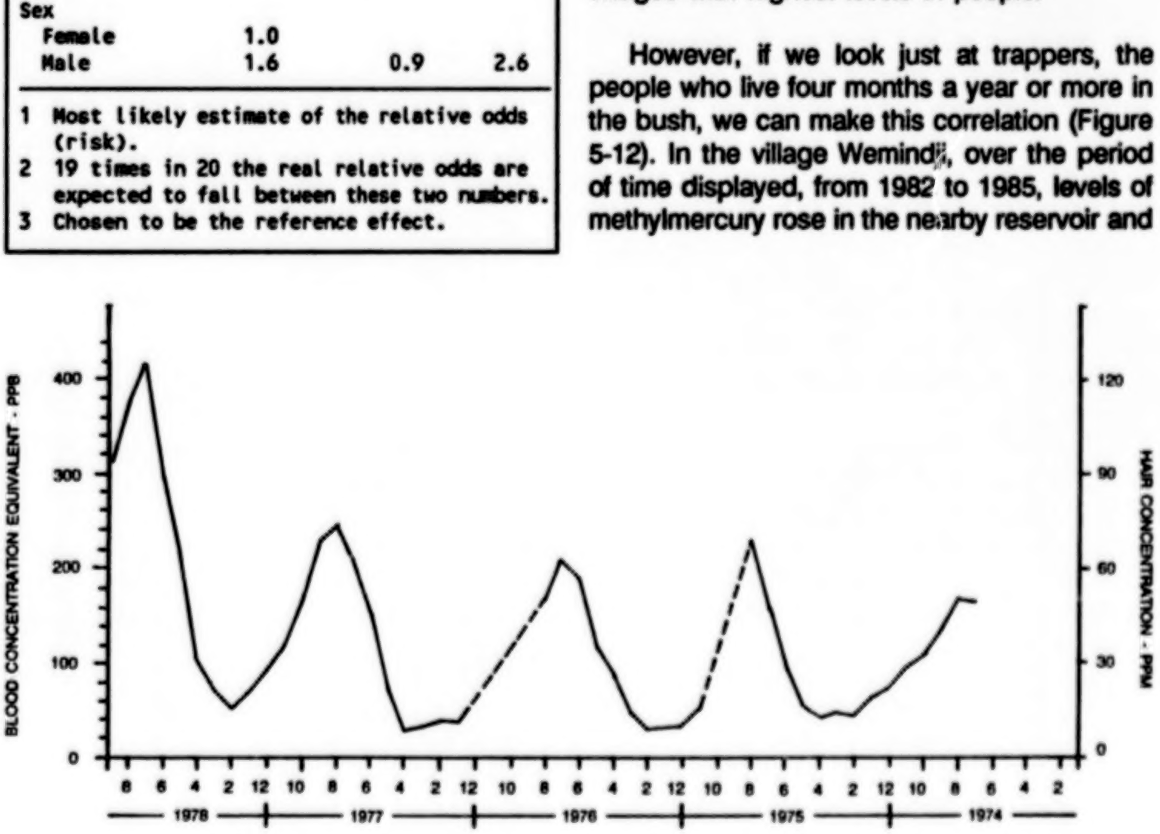

Figure 5-10. Seasonal variation of mercury level in a fishing guide from Grassy Narrows Reserve, Ontario, Canada (after Clarkson 1988).

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its fish, while methylmercury levels stayed the same in this other village, Great Whale River, which has the Cree name Whapmagoostui. So methylmercury levels in the fish were rising near to Wemindji shown here, while methylmercury levels in Whapmagoostui not affected by hydro-electric development were constant over here. We can see that levels in the male trappers 40 years of age and over, rose in Wemindji and stayed constant in Whapmagoostui. Fairly weak evidence of a relationship between levels of people and levels in fish. But it does appear that perhaps a relationship between hydroelectric development and methylmercury in people can be shown if you focus on the most exposed group. These numbers are however fairly small, there being only 20 or 30 people in each group.

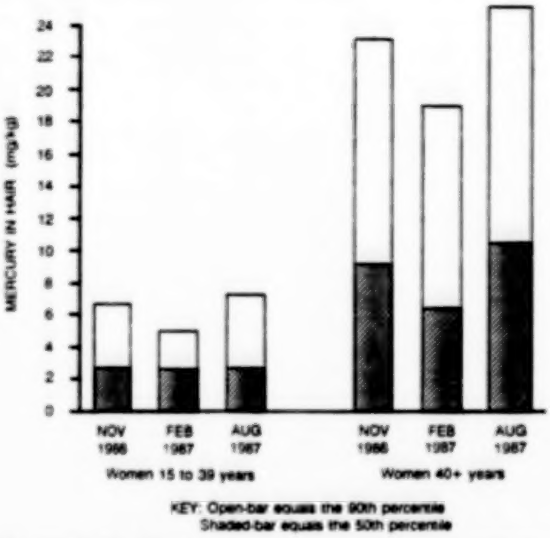

Figure 5-11. Seasonal variation in hair mercury concentrations among women from whom 9 cm length of hair was obtained, 1986-1987.

What are some of the other influences on methylmercury level? Figure 5-13 is from a paper by Margaret Wheatley, an anthropologist. She, with her husband, Brian Wheatley, who's with the Medical Services Branch (the Branch of the Canadian government that provides medical services to native people) looked at a community called Salluit in northern Quebec, a community of Inuit, or Eskimo people. The Salluit people had a very high level of methylmercury at one time. Each one of these lines represents a segment by segment hair analysis for one Salluit resident.

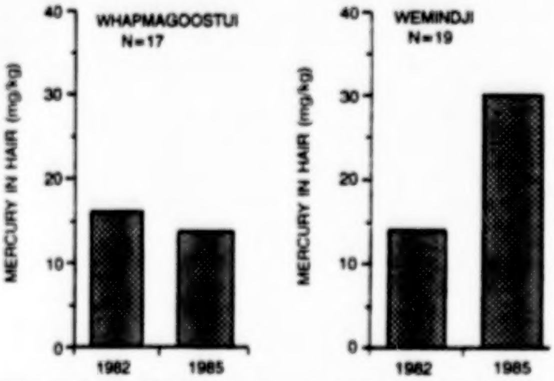

Figure 5-12. Median mercury concentrations in the hair of trappers age 40+.

Methylmercury showed a peak during 1977 in about May and June. It was much lower before and gradually fell after. The Wheatleys tried to figure out what had happened. They felt that consumption of lake trout, which had a methylmercury level of about 0.6 to 0.7 ppm, was responsible for this change in villagers. Lake trout, which hadn't been eaten in any quantity before by the village, was being eaten at the time that levels rose.

I'm not all that convinced by their evidence, but whatever happened some event led to a very great rise in methylmercury levels in Salluit people. The Wheatley's make the comment, although not supported by numbers, that they could find virtually no one in the village in 1979 who was still eating fish. Methylmercury levels in Salluit were extremely low in 1979. They felt that adverse publicity related to methylmercury led to the precipitous drop.

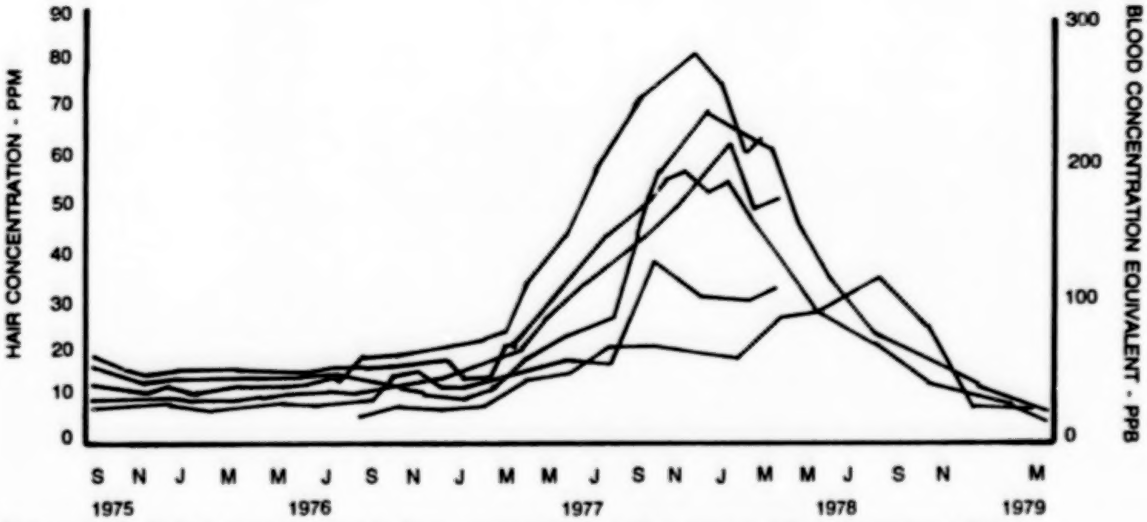

Figure 5-13. Sallult mercury levels from several long hair samples (after Wheatley 1982).

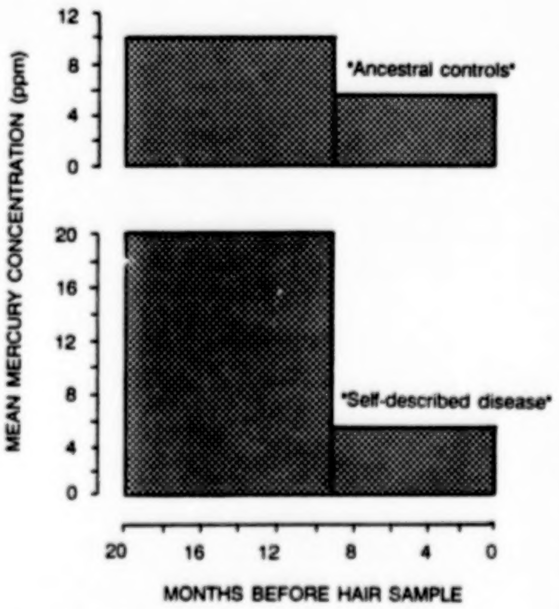

Figure 5-14. Mean hair mercury concentrations during two periods by study groups (after Spitzer 1988).

Here a situation involving the James Bay Cree demonstrates a similar effect. Figure 5-14 is from a study that was done by Walter Spitzer, of the Department of Epidemiology at McGill University. He was involved in looking at neurologic disease related to methylmercury exposure among Cree Indians in the southern part of the James Bay Territory. These studies were done during the 1970s.

At that time there was concern about exposure to methylmercury relating to chlor-alkali-process paper plants in the southernmost part of the territory in which the Cree live. Dr. Spitzer looked at people who felt that they had been made ill by methylmercury, and who described themselves as being fish eaters. He compared these people, the 'self-described disease' group to a control group, the 'ancestral controls' who were living in the same area, but who didn't themselves feel that they were being exposed to fish that were dangerous.

Using serial sections of long lengths of hair, Dr. Spitzer compared the concentration of mercury present during the interval from 20 months until 9 months before sampling and during the 9 months before their hair was cut for mercury analysis.

While this analysis does not appear in Dr. Spitzer's paper, if you pick apart one of his tables you can create a figure like this. It shows that for the self-described disease group,

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people who felt that they were being affected by methylmercury, that there was a precipitous drop in methylmercury levels between this aggregate 20 to 9 month level, and the 9 month to 0 month level. Clearly these peoples' exposure to methylmercury declined rapidly over a short period of time. The decline was much less in the control group, people who didn't themselves feel they were being exposed to something that was affecting their health negatively.

It looks to me as if publicity associated with this study, and the community suspicion that they were being affected by methylmercury, resulted in a big drop in consumption of fish, and a big decline in methylmercury levels.

I think that these are two examples where publicity may in of itself affect community-wide levels of methylmercury.

Back to the question of methylmercury in James Bay area freshwater fish. You can see that between 1978, 1982, and 1984 levels in both non-predatory and predatory fish rose markedly in areas affected by reservoir development (Figure 5-3).

Now, I'll try to compare that to human monitoring data for the village closest to the reservoir – Chisasibi (Figure 5-15). Since the Chisasibi human health sampling program really only started in 1983, we don't have very good pre-reservoir levels. But in 1984 there was concern over the sort of a distribution shown by this figure, where the mean level would have been about 18 ppm in people over 35 years. Between 1984 and 1986 there was a tremendous drop in levels among Chisasibi residents. You see that the distribution of levels was spread out towards the high end in 1984, while in 1986 it was much more compact, and moved over towards the zero point.

So between those two years, when levels of methylmercury in the fish were pretty well the same, levels of methylmercury in people as a group went down markedly.

That drop may be ascribed to publicity. Because 1984 was really the first year that there was wide-scale sampling of people in Chisasibi. As a result of concern and publicity, peoples' fish consumption likely declined markedly.

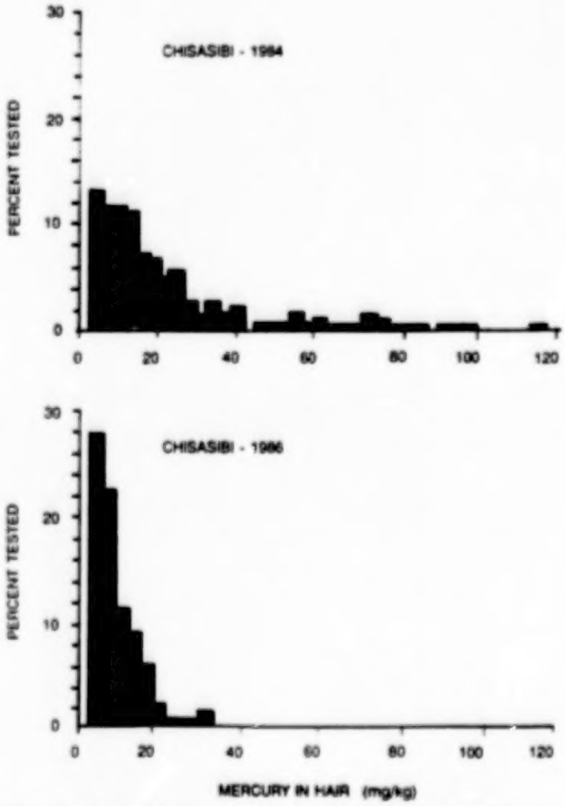

Figure 5-15. Mercury concentrations in hair of subjects 35+ years (Chisasibi 1984 and 1986).

Now, not only did that decline occur in Chisasibi between 1984 and 1986, but the decline occurred in all villages, (Figure 5-16). You can see again that just as this curve moved towards the left, towards the low exposure side from 1984 to 1986 in Chisasibi, so it did in all the other villages as well. So there was a spillover effect of publicity that was directed mainly towards

Chisasibi, but that in fact manifested itself in all of the villages. People ate less fish and had lower levels of methylmercury. While other interpretations are possible, this one does fit our observations.

So to summarize, looking at data on Cree people who were sampled year by year for methylmercury and trying to use these results as a mirror of what was happening in the environment, we find no clear global relationship between what was going on in the fish and what was going on in the people.

On the other hand, there are very clear correlates of exposure to methylmercury, that relate to sex, to age, and to the different villages. There are also very important seasonal considerations. There's a suggestion from our own monitoring program, as well as from studies conducted before, that village-wide publicity can have very important effects on methylmercury levels in people.

My conclusion is that people are not a particularly good monitor of what is going on in the environment, if our aim is to correlate levels of a toxin in people to a known environmental process.

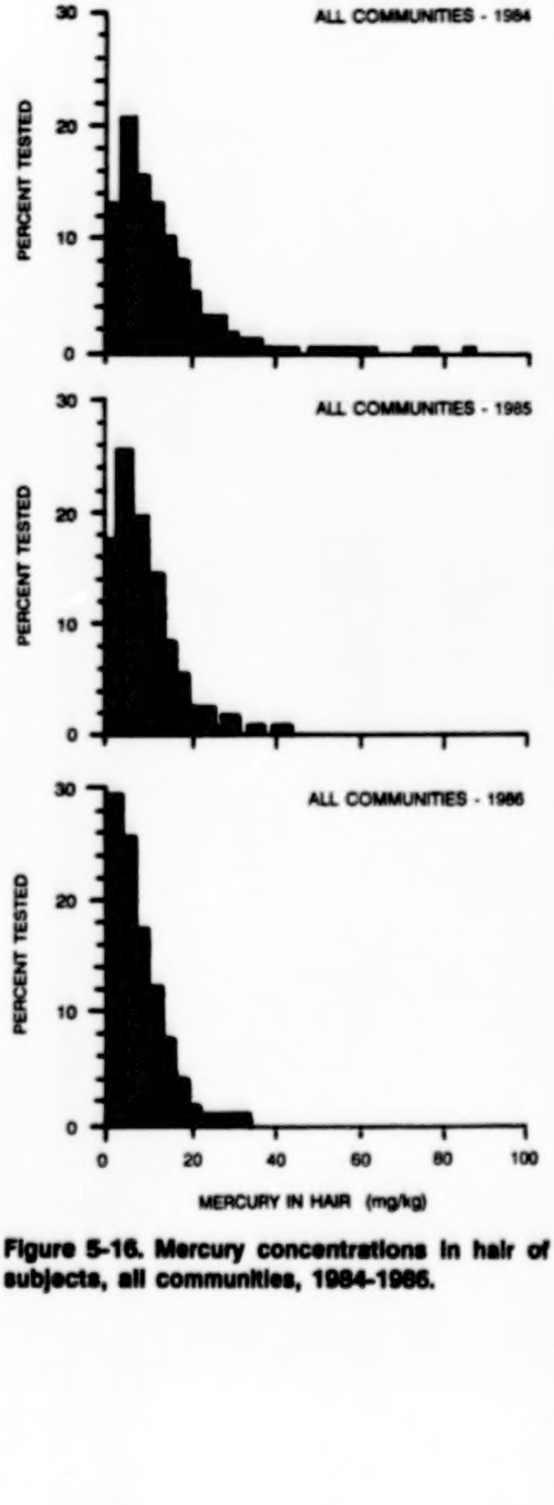

Figure 5-16. Mercury concentrations in hair of subjects, all communities, 1984-1986.

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HUMAN HEALTH SESSION

RECOMMENDATIONS

HUMAN HEALTH PROGRAM SESSION

SUMMARY OF RECOMMENDED STUDY ELEMENTS

Baseline

1. Independent segmental re-analysis of the 200 hair samples from Nome women to establish seasonal variation and maximum mercury concentrations.

2. a. Dietary survey to identify a high subsistence-consumers group among women of fertile age.
   b. Conduct segmental analysis for hair mercury content among high consumers.

3. Analyze for mercury in selected food species: Flesh and liver of bearded and ring seals, tomcod, and salmon.

4. a. Promote coordination among MMS, IHS, state agencies and local interest groups.
   b. Provide feedback to subjects on the results of these studies and on the benefits of a subsistence diet.

Monitoring

1. Conduct routine analyses for mercury in tissues of selected fish and sea mammals.

2. Conduct analyses for hair-mercury levels in high-exposure (hair-mercury >10 ppm) women, if identified.

Related Concerns

1 a. Lowest socioeconomic group is most depended on subsistence resources.

   b. Lowest socioeconomic group most likely to be impacted by adverse effect on subsistence food - either depletion of resource (e.g., king crab) or increase in contaminant levels.

2 a. Lowest socioeconomic group is least likely to be employed by mining operation.

   b. However, increased income would risk increase in alcoholism and attendant social costs, medical complications, and trauma.

3. Rapid expansion of mining could produce a "boom - bust" phenomenon with medical and social consequences.

HUMAN HEALTH PROGRAM SESSION

REVIEW AND RECOMMENDATIONS

David O. Marsh: The session on human health, I think, is a little different in that we are all human and we all enjoy some state of health. So whereas few of us, regardless of those statisticians, are experts in all areas, we're all experts on human health.

We have quite a disparate group, with different opinions; for some the glass was half full, and for others it was half empty. I think we've reached some sort of consensus: at least people said that they could all live with the things up on the blackboard, if that means consensus.

First of all we thought that the environmental impact statement should require or stipulate that baseline studies and a monitoring program for human health be conducted.

The baseline studies were discussed yesterday. You remember that 200 hair samples were analyzed from women of Nome, women aged 15 to 54 years. We believe that there should be an independent analysis by a separate laboratory of these samples. This should also allow segmental analysis, which would provide information about seasonal fluctuations and maximum concentrations. I think it's very likely that independent analysis would tend to confirm the results reported to date. An additional benefit of the segmental analysis, is that it might show that the highest concentrations of some of these is about 8 ppm. It is possible the seasonal fluctuation might show a maximum concentration of twice that, even those that were 4 or 6 ppm could approach or even exceed the lowest effect level for fetuses.

Secondly, we believe there should be a dietary survey. In order to identify a high-exposure group of women of fertile age, there are several possible approaches. Probably the most economical approach would be a simple dietary survey, asking about the numbers of meals per week of fish or sea mammals people consume. When one sees the result of that survey - how many, for example, more than three meals a week, more than four meals, more than five meals - then one could conduct further hair analyses on the higher exposure group, again including segmental analyses.

Regarding the independent laboratory analysis, there are several possibilities. We have two Canadian colleagues here, Tom Kosatsky and Anton Scheuhammer, either of whom could arrange for analyses, in the Canadian laboratory or through the University of Rochester, New York. And it would not be necessary to have the extremely sensitive method used by Dr. Crecelius. A quite adequate method would cost approximately 10% of the cost of the previous analyses per sample.

We think that after the dietary survey and identification of a high consuming group, if there is such a group, that there should be feedback to the subjects involved, including a positive statement about the value of the subsistence diet. We don't want the impression to go out that fish is poison or that a sea mammal diet is bad. We have no evidence to support that, but we have every reason to believe that there are many medical benefits from the traditional subsistence diet. So we should report the results to individuals but also give them positive feedback about the benefits of the traditional diet.

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There are also a couple points which other groups already mentioned. For our purposes they involve collecting data about the human diet, but that has already been suggested. And we repeat the previous comment about enhancing coordination between the various groups involved. There are still some groups that I've never heard of, I don't know what the initials mean.

In terms of monitoring, we have a very short list. We think that there should be periodic re-analysis of mercury in the fish and sea mammals mentioned previously. If a high-exposure group is identified, i.e., women of fertile age with hair mercury concentration greater than 10 ppm, we believe that population should be studied further; especially, if mercury analysis of seafood and sea mammals should show an increasing level. We don't want to keep testing the Nome population and we don't want to reinforce any fear they might have about their diet being bad. So we only suggest further hair sampling of that group under those circumstances, unless there should be warnings from the results of analysis of items of their diet, or perhaps hint from chemicals in the water column or in the lower forms of life that are being discussed for further monitoring.

Now, in general we all thought that any possible adverse health effect on humans from any of the trace elements is very remote. It's very difficult for us to link churning up enormous amounts of sediment with a direct impact on the physical health of the people of Nome. But there are several effects which, in a broad sense, are health-related effects. It was pointed out by people in our group, it is the lowest socioeconomic group, the poorest people, who tend to be most dependent on a subsistence diet and least likely to be employed. So they are unlikely to benefit from dollars and they are likely to be affected if subsistence diet is affected. For example, if crustaceans or king crab should be depleted, they would bear the brunt of such an effect.

Increased employment that would accompany the mining project would obviously bring more money into the area, and unfortunately, more money may be followed by a new group of social problems. There's already a problem with alcoholism, and the medical complications of alcohol would increase. Alcoholism is always accompanied by trauma, road accidents, and tremendous costs for social services. All of those things would tend to increase, and be indirect effects of the proposed mining.

And lastly, at the end of the project, suddenly everything will stop, and we will have the classical "boom-bust." Additional dollars no longer come in. The effects I've mentioned are there, but there are not necessarily compensatory dollars. If the poor people have felt the brunt of a depleted harvest of something like king crab, then that is left as an end point of the project. Again, the dredges have gone, the money is gone. So, this is a group of social, economic, and health related effects that we wanted to include in this summary.

Robert Smith: Are you proposing to measure those effects in any way?

David Marsh: Well, it's difficult to know. You may have noticed that they were on the transparency, under the heading "Monitoring," but I didn't give them a monitoring number. It's difficult to know whether they should be put down as Points 3 and 4 under the monitoring – or not. I put them in as a sort of an addendum of interest, in terms of health-related anticipated effects.

It is quite obviously an extremely difficult thing to measure. We've had talks about measuring all these chemicals, but when you get to social effects, it's a major project to actually measure something that you know is happening. I don't have a slick answer as to how that could be done or how it should be done. It would be complicated to link an increase of road accidents with the money coming in from the mining project. Yet, we know that this is likely to be one of the complications. Maybe other people would have some suggestions as to how those things could monitored or measured. They are relevant, but one tends to not think of them, and I don't think one can in a few minutes respond to how precisely all those very complex interactions would be measured.

Anton Scheuhammer: Did your group consider any health effects related to exposure to other things, besides mercury?

David Marsh: Well, we did, but we only have a small amount of background data. For example, I have no idea what the typical blood lead level is in women of fertile age from Nome. I do know that in a big survey in the United States a few years ago, the average level was about 10 μg/dl (micrograms per deciliter), which is within the lower end of the range for adverse fetal effect. It is very difficult for me to suggest a scenario through which the proposed dredging mining operation would increase the lead in the diet. I think one has to rather strain to make that connection. But I have no idea whether the current average is 10 or 4 μg/dl and that truly makes a difference. If it's getting up close to a critical level, then a small increment would be important. It's the same argument one might use with methylmercury. In terms of other metals, nickel, or silver, or chromium, or anything, including arsenic, I don't think any of us could come up with a plausible way – even using a lot of imagination – that any increase through the mining would endanger health.

So, actually, we didn't discuss this at great lengths because the group thought that there was simply an absence of any obvious risk. But there is going to be a lot of monitoring of metals in the other parts of the program, as already discussed and one of those is lead. I don't know whether that should be part of a baseline measurement of the human population or whether that could be left until such time as there is evidence from the water column or the sediment analyses that lead has been increasing. It seems to me that the possible effect from those other elements on human health is terribly remote.

In a sense, it would be nice and neat if one could indicate a plausible way that lead or arsenic might do something. But I think we know, to a much greater level of certainty, these less tangible social effects – the effects of money coming in and the bust at the end of the boom. They are less attractive (to study) because you can't depict them neatly, in a numerical fashion. I think that impacts from those elements, trace metals, are extremely unlikely. Do you see any of them as potential hazards?

Anton Scheuhammer: I don't see any potential hazards from metal exposure to humans. But, last year one of the reasons we recommended that studies be done on mercury, was that we thought there might be a potential problem. And why doesn't mercury, now, fall in with all those other metals?

David Marsh: I think it gets very close, it very nearly does fall in with them. If we had to gamble a few cents on whether the proposed mining would increase human exposure to mercury in any form, I think we'd all gamble that it would not. It is simply that the lifestyle, the diet, is traditional; that by all accounts there are individuals who do eat a surprising amount of fish and marine mammals. And even though the 200 hair samples really didn't reflect that, some of us are concerned that the other 78% of the women in Nome might include a group who do eat the way that residents of the area tell us people eat. So that's why we pick mercury out as rather different; we're not aware of any other trace element that is likely to be elevated in that population.

Eric Crecelius: Is it likely that the concentration of methylmercury in the hair of the natives in Nome will actually go down through the dredging operation, because there will be more money in the area? Will more people spend more time working (less time gathering) and buying more food from the supermarket?

David Marsh: Well, I think we are speculating here, and there's never any brake on speculation. My own speculation is that the ones who already have low levels are the people who have money to buy a mixed diet. And, they're the ones more likely to be employed and more likely to benefit from the dollars coming in. It's very likely that their hair mercury concentration would become even lower. Now I have no proof of this, but my suspicion is that, if there are people with high hair mercury concentrations, they are probably the people who are less affluent, who depend on the subsistence diet, who do not have the alternative of buying groceries in the store. They are not going to benefit from the dollars coming in. If anything, they are going to be disadvantaged. I think if there is a group at risk now, I suspect there is a tendency for that group to become worse off. I suspect that current group, which is clearly not at risk is going to be at even less risk. But this is purely speculative, who knows.

Matthew Iya: I just want to clarify and react to what the gentleman over there had to say. The majority of us that came down here are right now a working group, we are employed. But we still set seasons in which we participate in subsistence activities, whether we are working or not. As I described the other day, we hunt small marine mammals, larger marine mammals, then there is fishing and gathering berries, roots, or what have you, which we do throughout the season. Just because we're working doesn't mean that we deviate from what we have always done. There also is a native food store in Nome.

Helen Armstrong: To support what Matthew said, in some studies we have found that you often have both. Maybe unemployed people will be doing more subsistence hunting and fishing, but as Matthew said, they may be employed part-time because then they can afford things like – we see this on the North Slope – airplanes to fly out to fish camp for the weekend or a nice boat so they can do more marine mammal hunting. Sometimes you'll also find a lot of flexibility with vacations. We think only of vacations for three weeks, but you'll find that in those areas, people take really long vacations that will enable them to hunt and fish even though they work.

Ray Emerson: Is that the present database that we're presently looking at? Is it complete in the sense that we have an adequate connection between the resource background levels, consumption by the individuals, and the hair samples?

David Marsh: As far as I am aware, we do not have consumption data. If there is such data, I've not seen it. (Incidentally, one thing I learned today was that 200 women of Nome of

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childbearing age allowed their hair to be cut; however, 50% of the people refused when asked. So this also increases my difficulty in knowing how representative those 200 are of the target population of 900 women of that age group. Fifty percent refusal is a fairly high percentage. It is possible that there's nothing different about the 50% who refused, but it's also possible they're quite different.)

In response to the comment about consumption, I am not aware that there are any consumption data for that group. In our proposal, when we talk about a dietary survey, we mean a survey that would ask some simple questions about the number of meals per week of fish or sea mammals. The survey would include some additional simple questions about the type of food - whether it's salmon, walrus liver, or whatever - and an attempt to arrive at a rough estimate concerning amount.

Now when you get into amounts, I think one must appreciate that it is not easy information to collect; getting dietary data on amount of food is a sophisticated business. If you ask people what they had to eat every day for the last few days, and how much of each thing, they are likely to give you very inaccurate responses. And it's even more difficult in a society where they tend to have a common dish and each dips into it, rather than each having an individual plate. It is a little bit like being in a bar and having somebody fill your glass up when you're not looking: you don't know how many drinks you had at the end of evening.

So, my view of a dietary study would be to aim primarily for simple things, like the number of meals per week of fish or sea mammals, some information about what items of food are eaten. I would be a little bit skeptical about the data concerning the amounts, but in a crude way, that would separate probably the highest consumers from the lowest.

Ray Emerson: If we had that information, could we still have some major gaps as to what those food resources actually have in mercury content? If indeed, they're low. We had estimates on salmon yesterday, I think, which were maybe an order of magnitude lower than might be expected. We would need some information, I assume, to correlate with the different food resources, what the mercury levels were; not only the different food resources, but what parts are being consumed. Liver, kidney, and muscle tissue are going to be quite different.

David Marsh: Yes, one of the items on the transparency was the suggestion that we analyze the major food items, both of seal and walrus, both the edible flesh plus the liver, maybe even the kidney, although the kidney is largely inorganic mercury. Now there may be all sorts of information in various agencies, measurements from the last year or two, but I'm not aware of these. So we do have a tremendous gap.

Yesterday we talked about what seemed to be a discrepancy between the rather low mercury levels in hair and what we understood to be the dietary habits, including a lot of fish and sea mammals. Now if salmon, which is a major article of diet for a number of months of the year, is really as low as 0.03 ppm (that is a very, very low level for salmon) that would help to explain part of that discrepancy. But it would be very surprising if the major species of fish and sea mammals all had lower levels around Nome than anywhere else. At the moment I can't quite bridge the gap between the very, very low levels in hair and what the local residents tell us about how much they eat.

Ray Emerson: I think it's very encouraging that the numbers that we do have are low. So far the situation looks good and I certainly wouldn't want to be credited with shutting down the subsistence lifestyle in Alaska. What I meant – in terms of the importance of the database and the education factor that has affected the Canadian population – was that with the right information I'm sure we'll be able to pursue almost a full subsistence lifestyle, hunting, fishing, and so on. We can probably maintain a higher degree of health in the subsistence lifestyle than I lead in going to Safeway, which unfortunately doesn't give me any indicators of the levels of trace metals that I'm shopping for. So it looks good so far, I think.

CHAPTER 6

STATISTICAL AND SAMPLING TECHNIQUES SESSION

Woollcott Smith, Ph.D., Session Chair
Robert Smith, Ph.D., Assistant Session Chair

GENERAL SAMPLING CONSIDERATIONS FOR BASELINE SURVEYS FOR OFFSHORE MINERAL MINING

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Time series models can help to define the issues in baseline sampling for monitoring and tracking changes due to offshore mining. For the purposes of this discussion we will assume that two qualitatively different impacts are to be studied:

A. The physical alteration of the sea floor and the consequent change in benthic communities.

B. The resuspension of trace metals and the bioaccumulation of these metals in the food chain.

Impact A is characterized by a rapid change after alteration of the sea floor, followed by slow recovery, as measured in years or decades. Impact B as measured by increase concentration of trace metals in organisms or surface sediment is characterized by a slow rise in concentration followed by a decrease. In the case of Impact A the most important question is the rate of recovery of the area and the total area affected. For Impact B the important question is the maximum increase in trace metals in organisms.

In this abstract I describe some sampling ideas that play an important role in decisions about replication, sampling frequency, length of baseline study, and the use of composite samples.

A time series method known as intervention analysis is used to estimate the impact of a known intervention (i.e., mining) over time. Lattenmaier *et al.* (1978) have used these techniques to determine the optimum baseline period for environmental surveys. For the offshore mining baseline study the implications are straightforward. For Impact A only a short baseline, one year, is necessary, since the impact signal is very different from the natural variation from year to year. For Impact B, a relatively long baseline period is essential since the impact signals are similar to the natural variation from year to year.

Within-year sampling can be carried out once or at fixed time intervals within a sampling season. Smith (1984, 1978) and others have investigated the frequency of sampling to estimate a mean property of an environmental time series. Solving the problem depends on a general knowledge about fluctuations of the process with a season. If there is little or no fluctuation within a season, then a single sampling period is most efficient. In general, marine environmental surveys have tended to sample too few time points while overemphasizing station and replicates samples.

Total cost of a survey is often driven by the number of replicate samples at a single station, since chemical and biological analysis for samples is the major cost. Replicate samples play two roles. The most important role is the quality control and evaluation of the integrity of the sampling process. This is particularly important in measuring compounds at low concentrations. The second role is the reduction of within station sampling error. An optimum allocation procedure, whose only objective was to estimate the mean over space and time would reallocate all replicate samples to additional stations or time periods.

Within-station replicate sampling error is often used to evaluate the accuracy or statistical power of environmental surveys. This is what Hurlbert (1984) has called pseudoreplication. A more

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accurate error analysis would include natural between station and between time period variation as well as within-station replicate error.

The goal of a baseline survey for offshore mining is to estimate mean effects, for example mean concentrations of trace metals in tissues. Statistical sampling procedures, designed to achieve a fixed statistical power, often yield very large sample sizes requirements due to natural variation between stations and time periods. The cost of the chemical analysis of all samples is usually prohibitive. A straightforward solution to this problem is to physically composite subsets of samples and then do the chemicals analysis on the subsets of samples. A carefully planned composite sampling program can greatly reduce chemical analysis cost while still providing enough information to evaluate statistical sampling error (Boswell and Patil 1987).

Composite sampling is now in wide use in environmental monitoring; standard environmental texts give detailed procedures for estimation and hypothesis testing using composite sampling procedures (Gilbert 1987).

Composite sampling programs, while theoretically appealing, are often difficult to carry out in practice. Compositing, homogenizing and splitting the samples can introduce errors (Gilbert 1987). This is particularly true in trace metal analysis where sample contamination is a problem. Any composite sampling program should be evaluated in the laboratory before it is used in a field sampling program.

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PRESENTATION AND DISCUSSION

First I'd like to give my definition of a "baseline study." For me, a baseline part of the survey is doing whatever you need to do to get ready to monitor the effect. If it means measuring ambient conditions, that's part of the baseline. If it means tightening up your quality control, that's part of the baseline. If it means devising new measurement techniques, that should go into part of the baseline study. The criterion for judging a baseline study is "at the end of the baseline period do you have the data and measurement resources available to detect the impact in the monitoring stage of the study?"

In this presentation I will briefly outline some important statistical ideas that will help in the planning of this study. These include: intervention analysis, time series sampling, composite sampling, station replicates and what we mean by "pseudo-replication." I'll try to do that in a few minutes, and then we'll open up the floor for discussion.

For large environmental studies the cost and efficiency of a program are determined by the answers to the following set of questions:

- How many times should we carry out this survey?
- What is the tradeoff between accurate surveys at a single time point and knowledge about the dynamics of the system?
- How important are baseline surveys and pre-intervention surveys?
- How long should a monitoring program continue after you start it up?

These are exactly the sort of questions we have here today.

If we look at dredging, we have two different kinds of impacts (Figure 6-1). The upper figure represents the signal. There is a baseline period (the pre-intervention period), after which we have an intervention. This might be introduction of suspended matter, etc. We have two kinds of hypothetical signals; one is a sharp rise right after dredging, and then a decay. The other one would be a slow rise, for instance, bioaccumulation of trace metals.

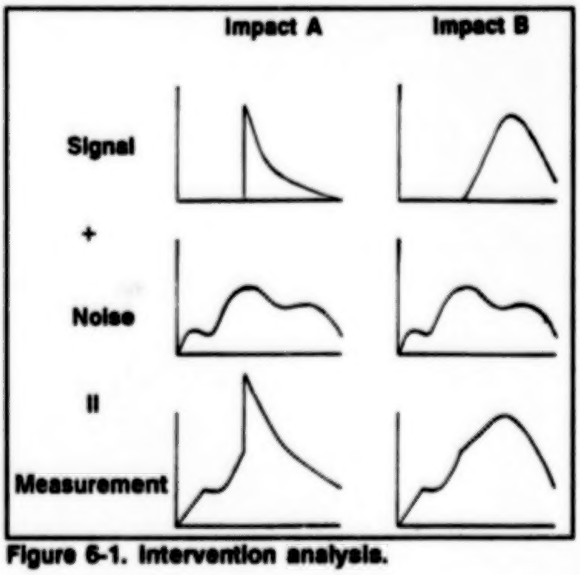

Figure 6-1. Intervention analysis.

The middle figure is natural background noise. The lower figure represents the actual field measurements: the sum of signal plus noise. The Impact B signal has many of the same characteristics as the background noise, thus, it will be hard to extract that signal from the background noise. For Impact A the signal has a single very sharp increase that is clearly different from the background noise. The signal from Impact A can be extracted from the background noise.

For Impact "A", you do not need a very long baseline survey, because all of the information is gained at the point of the immediate impact. Therefore a long baseline before the impact is not very useful. In Impact "B", a longer baseline would be useful, but you may not be able to extract the signal anyway. Intervention analysis provides the statistical tools for survey design and estimation of these kinds of models.

An important goal of a base line survey is to estimate the mean concentration over a season or some other fixed time period. In this situation we must decide how many sampling points to take over this fixed time period. Smith (1978) and others have used straightforward analysis to answer this question. To optimize the number of sampling points you need a description of the variability and the sampling costs. I think we can get this kind of input. You need the random sampling variation at a single time point. You need the variance of the natural process. You need something like the decay of the natural process. That is, if you put something into the system, how long does it take to decay; usually you can get a rough estimate of that just from the dynamics of the system you're looking at. You also need some cost parameters, the cost of analyzing a sample and the cost of a new time point. From this information you get something that approaches an optimal sampling frequency. The analysis is fairly robust to modest changes in any of these parameters.

How many replicates should you take at a single station? If you're only interested in estimating the mean over an area, and that's all you're interested in, then you take one sample at each station and put all your sampling effort into increasing the number of stations. But that's really not our only interest here. We're interested in quality control. We're interested in understanding the sampling process. So when people advocate taking more than one sample at a single station, they're advocating looking at the sampling process, looking at the technical process of compositing the samples, and laboratory error. I agree that should be done. EPA assigns about 20% of the costs of any survey to something they call quality control. Part of that quality control cost is the cost of taking the replicate samples. So the 20% figure might be a reasonable guideline for this program.

The only way to decrease sample size and cost is to increase the signal to noise ratio. However, this is not easily done. The argument about whether to sample the sediment or the water column illustrates the point. Eric Crecelius wants to sample the water column, because he knows the water column is well mixed, and the sampling error or noise will be small. So he has a small sampling error, but when he looks at the trace metal signal, he discovers that the trace metals are also well mixed (i.e., small signal) and thus the signal to noise ratio is unchanged.

When we go over to the sediment, there's at least some argument that we would expect the difference between the control and the experimental area to be higher. Thus the sediment signal is larger, increasing the signal to noise ratio and reducing sample size. However, when we look at sample variation within the sediment, because of differences in the fine sediments between samples, we see that noise is high; that's a negative. High noise will decrease the signal to noise ratio and increase the sample size.

This argument is framed in terms of the differences we expect to see, and the expected variability.

Often optimum sample size calculations come up with very large sample sizes. Sample sizes of a many thousands are not uncommon. The question is, how can we possibly do that many analyses? The answer is, you can't. So a system, called composite sampling, has been devised. It is in wide use in other areas of environmental science, but not in marine sciences.

An accurate mean over space and time can be obtained from many samples by physically compositing many samples and then making a single measurement. Bob Smith mentioned that Gilbert's environmental statistics book has a chapter devoted to composite sampling. So there's no reason why a well-defined baseline program could not use a sampling scheme like this.

If you're doing this kind of compositing, particularly with trace metals, you have to make sure in a baseline program that you understand what's going on. If you're doing a lot of mixing, there's a chance of contamination. This should be carefully checked out in the laboratory, before it is implemented it in the field.

STATISTICAL AND SAMPLING TECHNIQUES SESSION

RECOMMENDATIONS

STATISTICAL AND SAMPLING TECHNIQUES SESSION

SUMMARY OF RECOMMENDED STUDY ELEMENTS

The statistical sampling recommendations can be summarized in three general strategies that can be applied with minor modifications to the sediment, water column, food chain and habitat alteration studies. The recommendations are not intended to apply to the human health studies.

Strategy A. Within-season control points

The period just prior to mining in June of each year should be used as a time control. Sampling should be conducted in this period and again in August. This within-season comparison will avoid confounding with long term effects not associated with mining and increase the power and efficiency of the monitoring study. The baseline study should use the same sampling strategy.

Strategy B. Composite sampling

Local variability can be reduced by using 10 random samples per station. The samples can be composited and a single analytic measurement performed. The only extra cost involved is the extra time on station to collect the samples. Compositing procedure will result in a three fold increase in the accuracy of the between station comparisons within a year.

Strategy C. Limit replicate chemical and biological measurements

Analysis of replicate measurements within a station are useful for quality control studies and for investigating the statistical properties of the sampling procedure. However, the number of stations and the between station variability determine the power and efficiency of the baseline study. The cost of analyzing replicate samples should be kept below 20% of total analytic costs.

STATISTICAL AND SAMPLING TECHINQUES SESSION

REVIEW AND RECOMMENDATIONS

Ray Emerson: One of the purposes of our monitoring program, in terms of this activity, is to have information that is going to yield answers to questions about what we should do with the operation, if anything.

If we do see some problems, the question that comes to the MMS Operations Chief is: can the operation be modified, or should it be shut down? We have to say in the stipulation for monitoring, in this type of activity, that the monitoring is effective. The "effectiveness" is the ability to have the operator modify his dredging activity to minimize the perturbation that we've detected from our monitoring program. The way that we will detect a significant effect, at which point a decision needs to made for modification or shut down, is going to depend on how we determine significance. Yesterday, we started out with the 2 X 2 table which was characterized as too simplistic at one point.

Woollcott Smith: The question I was going to ask is - and this would address all four sessions that went before - suppose we see some perturbation in the system. How are you going to use the data that you gathered to help you understand that perturbation, and show that it is significant? I think that is more or less the objective.

I'm particularly worried about the once a year sampling plan from the sediment discussion group. The signal, if any, from the dredging operation will be a yearly signal, generated during about hundred days. The dredging signal is confounded with a seasonal signal that's very strong. You have to understand both those signals in order to be able to pick apart whether or not there's any effect.

That's why I particularly like the design proposed by the bioaccumulators group. They acknowledged that dredging will generate this kind of signal in the system. I have not seen that acknowledged by any of the other groups.

I would now direct a question to the three groups: How are they going to use their data, collected over the next three years, at some cost, to help understand what is happening, if and when the dredging system is put into place? Let's start with Bob.

Robert Smith: I think that they would probably ask you, how are you going to use their data? How would you use their data to get that information, because they aren't the statistician.

Woollcott Smith: I'm telling them right now, that I don't think they've designed a very powerful system for detecting that particular signal.

Robert Smith: Let us assume, since all the different groups are going to be coordinated, that if one group samples three times a season, then probably all of them will. They are going to be out in the field anyway, and that's the main expense. Assume that is not a problem and that we do have the correct data that we need. Do you want to say something about the basic design of the model that is usually used in impact studies?

Woollcott Smith: Why don't you go ahead, Bob.

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Robert Smith: This is the basic design where you sample two areas, one an impacted area and one a non-impacted or control area (Figure 6-2). You want to sample both over time. The theory is that, if there is an impact, the usual relationship between those two areas will change.

For instance, suppose this was the amount of mercury or something in the surface sediment. Let's say there was an impact between this time and another time. Well, you would have a divergence of the amount of mercury in the two. You can see that it would be better to have a before time period, so you could see how they naturally varied in relation to one another, the control and impact stations. But that's basically the idea, you just see how it behaves in relation to a control. If there is a divergence or even a convergence, I suppose, you have to measure it against the natural variability. That is the error element in the model.

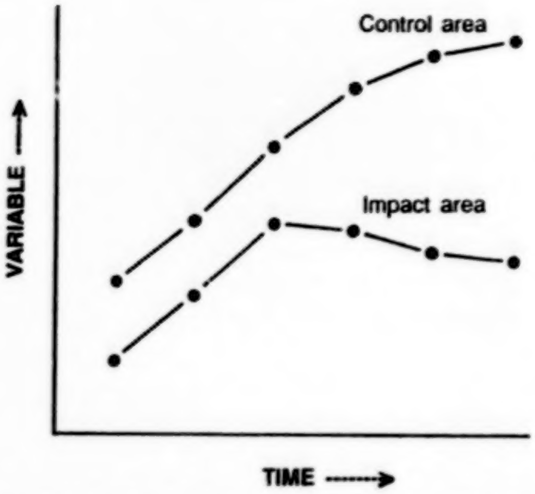

Figure 6-2. Illustration of effect on control and impact areas over time.

Woollcott Smith: Let me point out one thing. This monitoring program is different from other programs in that we do have this peculiar signal – a hundred days on, 250 days off. That's different from the kinds of signals that you see in most other pollution monitoring programs. Therefore we have two different kinds of controls: a time control and a space control. The object of the figure is to show how both the time control and the space control fit into an analysis of variance setup. We also have something that looks a little bit like a "new time starting point" every season. We have to factor that in, because that's what going to produce the power.

John Trefry: I would like to mention that the water and sediment group did discuss temporal change. The basic question is what do you select as a time period? Is it a month, a year, a decade? You have to be realistic in what you select for a time frame, therefore we selected a year.

I would also like to comment on sediments. If you're looking at the concentration of a trace element in sediments (natural sediments, with no anthropogenic loading) you can normalize that metal to something like aluminum, or iron, or scandium, and it will be a straight line. The bioaccumulation group may have a different problem, and therefore their seasonal concerns are more valid than the sediment chemistry per se. We can normalize that out.

Tom Kauwling: It seems to me that we're talking about a matter of scale here. You're looking at each year as a new situation, where you have your before, during, and after monitoring. In the sediment group designed a baseline study, where only the before and the various impacts come at yearly intervals. And each of those is a sampling period. So, we could just tag on each of these, and the baseline may or may not fall back to what it was, and the impacts may accumulate from year to year. And you're just looking at the one window. There's nothing wrong with that. But it seems to me that if you add year after year of data, you're just extending that time series. Is that fair?

Robert Smith: Yes, you could look at different levels, different time scales, do separate tests on those if you want.

Woollcott Smith: What is driving this system? What is forcing me to ask that question is the frequency of the signal. I have to do my measurements frequently enough to detect the particular frequency of a signal. There are a lot of other signals out there that have yearly long term trends. What we want to do is to use a design that's particularly sensitive to the signal that we're trying to detect. That is why if we just go out in August, what we're going to see is a trend. The problem then is to decide whether that trend can be assigned to dredging operations or to some other forcing function. The best way to separate out the natural variation from the kind of variation that's being forced by the dredging operation is some sort of within season sampling, with a control station. So you have to have the control in space, and you have to have the control in time.

Ray Emerson: When you say "control in time," do you mean at that point in time for comparison? Initially, we might expect to see quite a divergence or signal. Through time (probably several years) we might expect a natural, maybe successional recovery. Those are different points in time, but the overall time trend needs to converge. That's easy enough if we look at the critters. We have a feeling for what could be recovery time, what's involved with recruitment, and so on.

But the real problem, I think, is if those two trends don't converge, or maybe begin to diverge, maybe that's your point. Where is your decision, do statistics tell us where to make that decision?

Woollcott Smith: I have no problem with a yearly sampling program in order to measure recovery from an already mined system. We are talking about 300 acres a year that are being mined and disturbed. Certainly if you want to follow recovery, some sort of yearly sampling program would be reasonable. What I'm worried about is detecting the medium field and the far field effects – effects out to 1 or 2 km or 3 km. We don't seem to have a plan that would detect those kinds of effects. I'd like the other session chairs to respond. How should we proceed?

Robert Smith: I think that one of the problems is that we don't know yet where the impacts are going to be, or exactly where they're going to mine at all. So people are trying to cover the area. Some stations may become impact stations, some become control stations. Then there will be that gray area with those that are in the middle, and a couple kilometers away.

Donald Hood: I can see the baseline study on a simply yearly analysis, which the water quality group proposed, might need a little more time during the summer to see if in fact there are seasonal cycles to the baseline. I don't see anything particularly wrong with that. And then I do see the power that you're talking about, Woollcott. If you've got a signal out there that hits you once every year, you certainly don't want to miss the signal if you're looking for an effect. I suppose, however, that some people would be willing to say there is no effect, if after a few years you can't see a trend. It's probably a matter of what we want to call an effect.

Woollcott Smith: Well, let me suggest that every statistician that has looked at the temperature time series, which is essentially a yearly time series, has said they cannot see the effect of greenhouse warming. It's hidden in the noise. Does that mean we want to ignore that? If we could do an experiment on the atmosphere that would detect that effect, we'd certainly want to do it. All I'm suggesting is that we have a way of detecting the effect here. And we probably want to take advantage of it.

Donald Hood: You're speaking particularly in the monitoring phase. What about the baseline?

Woollcott Smith: For the baseline you need to investigate the seasonal fluctuations in the period between June 15th and September 15th.

Donald Hood: I think that's very relevant. We talked about "interannual" sampling and effects, because it's very difficult to get back and conduct seasonal sampling. However, Nome is sitting there with a lot of opportunities for ships and people to get out into the field during the baseline and pick up such data. That would provide the opportunity to see the seasonal effects while an operation is in hand. I think that's a very good suggestion, and one that could be accommodated. Do you see anything wrong with that over there, Eric?

Dick Prentki: I'd like to address that for the water quality section. We're looking mostly in terms of the baseline effects, not in terms of monitoring. Once we do have a dredge out there, there is going to be a coalescence of any monitoring that MMS is doing with what the EPA is going to require.

Now, the way they have done it in the past is much more in character with what you're looking for. They're going to have them go out and do their sample every day at a control site, and behind the dredge. Well, at least now they start every day early in the summer. And then every once a week, once a month. But they do have early, mid, and late season samples. You're going to be getting some of information you want there, as long as the quality of the work is good.

Woollcott Smith: What's the dimension? What are we talking about, kilometers?

Dick Prentki: Well, we thought that they may need to go for control to 10 km away from the dredge site. And maybe use one of our reference locations that we're proposing for baseline.

Donald Hood: How frequently would we be doing the 10 kilometer away stations? It wouldn't be daily?

Dick Prentki: I don't know. They certainly could do it three times a year.

Donald Hood: Well, I guess that's what's being asked.

Dick Prentki: But that's really a different monitoring program, which is going to be in existence also with somewhat different purposes. They're going to be looking at some of the near-field effects of that. There's no reason why that couldn't be incorporated that way with the seasonal trend. I don't think it's going to be a problem. The baseline program for water quality is going to set part of what we're going to look at and set the groundwork for it. We're getting a mid-season point, and then you can see how you're progressing across that with your seasonal stuff at two sites.

Woollcott Smith: The program is designed to detect a signal from that dredging operation at a considerable distance, 10 kilometers. I think it may not be an important signal, in terms of biological effects, but you should be able to detect that signal. I would like to see a program that can detect that kind of signal, because then you have some confidence in your sampling program.

Tom Kauwling: You keep using the word signal, and I get the impression that you're thinking in terms of the plume signal. I'm thinking in terms of sediment signal, which Paul showed us yesterday is there for at least 2 or 3 years. It doesn't seem to me that there's any reason to necessarily sample those two different signals at the same frequency. Or perhaps an annual program for sediment work, three times a year, before, during, and after for the plume itself, because that signal was clearly gone at the end of the summer, once the dredging was over. I can't see why a program would have to have uniform sampling throughout, with all the different components. I don't know how anyone else feels about that.

Woollcott Smith: I'm not suggesting that. I'm suggesting that it may be important, or interesting anyway, to have a sediment station that is at some distance from the mining site where you look very hard at whether or not you can detect a signal. I wouldn't want to duplicate that on 16 sampling grids or whatever.

David Marsh: Well, there was a request for some response from each of the four sessions. I'm afraid as far as human health goes, we have to start speaking a totally different language. Humans really are a bit different from chemicals, water columns, and sediments. I think it's impossible to apply this signal concept. Humans have the ability to make decisions, crustaceans probably "stick around" despite what happens. It's so different, we really have evolved a little bit. Nome people are putting away supplies for the winter, Matthew has 25 salmon in his freezer or drying them, so there is no distinct signal. Humans interfere with any possible signal. We have all sorts of different noise: We probably have a seasonal variation for methylmercury, according to the hunting season, but it is not a consistent seasonal variation. We were told yesterday, that during the past year the walrus decided to go somewhere else. Sometimes there are hundreds, sometimes a dozen. There are these tremendous natural fluctuations – nothing to do with mans decision making. People also make decisions about what they're going to put away, so they don't have to survive from hand to mouth. There's another quite different, probably gradual cultural change so the traditional life style is probably falling away. The seduction at the supermarkets, and the easier meals, that's superimposed. I'm afraid, as far as the human health session is concerned – whatever statistics you apply have to be without this so-called signal.

Concerning any possible impact of the dredging on trace elements affecting human health – we have to really stretch our imaginations to find any such link, and it is minute compared with all these other factors. There is no hope, that I can see, for any such signal coming at any point in terms of direct relationship to human health effects. If other people in the group have other opinions, or think that it could fit into the scheme. I personally just can't see it fitting in.

Cleve Cowles: Maybe we don't understand what you mean by signal. It seems to me that if "you can't detect a signal," then you are saying "you can't describe change." Perhaps then it is futile to consider monitoring for humans. In the Human Health session we discussed ways to perhaps use surrogate controls, for example, much as the other sessions have attempted to do. When you expect extreme variability, you try to build in a control, perhaps a quasi-experimental design. In essence, if you can't detect a signal, then you're saying you really can't monitor.

Tom Kosatsky: I'd like to speak to that point as well. I agree with everything that Dr. Marsh said about the difficulty of detecting effect. But I would also agree with Dr. Cowles' comment. If the likelihood of detection of effect is so low, and the signal is so likely to be lost in noise, such as seasonal variation, dietary change brought on by publicity, selecting some foods one year and not selecting them the next, selecting foods from one area one year as opposed to another area the next. Since the likelihood of detecting effects related to dredging is so minimal, then it seems to me that for purposes of monitoring, there's little reason to sample hair or anything else in humans as a monitor of what this dredging process is doing. Therefore I would recommend against further baseline sampling, and against serial sampling of humans as a way to show how dredging might be influencing human health, insofar as uptake of metals is concerned.

That doesn't mean that, for purposes of public health or public reassurance, especially given the adverse publicity about mercury, that sampling would not be worthwhile. Sampling in Nome this year has been very much worthwhile. Individuals sampled should be reassured, based on those results, and the community should be reassured if, in fact, those results turn out to be as low as they appear to be, based on other laboratory confirmations. I think that we should use those results for reassurance. Any human monitoring program that we consider might make sense from a point of view of reassurance, but makes no sense at all from the point of view of detection of change related to the dredging process. So I think that monitoring, based on human measures, is really a no-go.

Terry Haynes: Dr. Kosatsky, I have a question. Are you suggesting that if monitoring of marine mammals and certain fish species is done, and that if monitoring over time indicates increased mercury levels in those species, that we should not consider taking a close look at high risk human groups who consume those resources?

Tom Kosatsky: Of course what you say is true. Were the proposed operation to be different, for instance, were there to be metallic mercury on the barge, and that mercury released into the sound, or the barge submerged and mercury released in catastrophic fashion, or if a far larger area were going to be dredged, then the validity of human monitoring would be clear. But as the operation is currently defined, I think that the risk of significant uptake of mercury by humans, related to dredging, is so slim that monitoring makes no sense. Now, if I'm wrong, and in fact there is accumulation in the food chain, this will be revealed by continuous monitoring of the marine environment. Both from the point of view of looking at the effect of dredging, and from the point of view of public reassurance. Foods that relate to the dredging area, and that people may be eating, must be monitored in a continuing fashion.

If in fact there is an uptake of mercury into the food chain, then at that point, from the point of view of public health, we would need to identify persons with high mercury levels, who should be warned to change their consumption patterns. We should also change the dredging operation itself, limiting dredging so as not to allow for the entry of mercury into marine organisms, and into people. At that point, human monitoring should come into place.

But with what we know now, I see more negatives from a point of view of ongoing monitoring, such as diversion of resources from other health issues, and the over-medicalization of an issue which really is of little current health importance to people. I see more negatives coming from it than positives.

Helen Armstrong: What would you suggest as a level of mercury in the subsistence resources that should initiate concern, since we won't know what the consumption is for Nome? We don't know what they are consuming, so it is difficult to know what levels are dangerous.

Tom Kosatsky: I think that if there's more than 0.5 ppm in fish, or in marine mammals we know to be consumed (walrus, seals, etc.) then there's some reason for concern from the point of view of public health. That doesn't mean that it's related to the dredging, just that it may be a public health issue.

Helen Armstrong: I agree. But yesterday I think Tony said, 0.2 ppm was the level they had chosen as their level of concern.

Tom Kosatsky: It depends on levels of consumption, if the levels of consumption are likely to be low, then 0.5 is acceptable for the community. I would agree with the 0.2 ppm value if it was found in food items that were widespread. For example, if salmon, something that we know regularly and to be broadly consumed, came out at 0.2 ppm, then I would issue warnings for women contemplating pregnancy. The data would have to be examined on an ongoing basis. If anything related to dredging even approached those levels, then I think it would be essential to get an idea of how much food is consumed with respect to the area dredged. Even now, I think it would be very useful to encourage the Indian Health Service to have a subsistence food survey, similar to what was done in the Bethel area, to determine what foods people are consuming. We can make more sensible decisions after examining that information.

Ray Emerson: It seems like we have two issues. The first is establishing a baseline as to determining the sensitivity of the population, and second is a concern about any contribution to the mercury issue through the food chain. Are you basing your statements on the fact that you're comfortable with the present database, and it appears to be so far removed from what might be a sensitivity level, that's your decision point? Do we have a big jump here in terms of what dredging might contribute, or does that database need to be refined to make that decision later on? It seems to me if indeed the population were close to a threshold or a sensitivity level, then it would behoove us to monitor the population along with the respective marine food sources.

Tom Kosatsky: Again, I think that if mercury levels in the population were near levels of concern, then it would make sense to monitor the population in terms of public health, and if necessary to issue warnings to women who contemplate pregnancy. But because of the difficulty of detecting the effect of dredging on uptake of methylmercury in a free living population, who might or might not refuse to participate in a survey, who change their food consumption patterns without telling the people who are taking the hair samples, I think that, for purpose of environmental monitoring, it really is not feasible and not useful to contemplate ongoing surveys of human metal uptake as a mirror of what's going on with the dredging process. But from a public health point of view, particularly if there were important amounts of methylmercury in subsistence foods, either related to dredging or at the baseline, then I think it makes sense to offer screening to people.

Ray Emerson: First issue. Are you comfortable with the present database as to a sensitivity level at which that community may be exposed to?

Tom Kosatsky: Well, I'm comfortable with a monitoring process that includes marine mammals and fish that are likely to be eaten by people. To me that's the most important part of the monitoring program. The issue of monitoring the people themselves, whose levels can change from year to year is open.

Ray Emerson: But that's only half the story. If you know the levels within the food resources that people are going to be eating, it only makes sense if you know the sensitivity of the consumers. You only have half the story if you're just looking at the content of the food resource.

Donald Callaway: The question asked this morning was "Are we confident with the results that we've gotten?" The answer is maybe. The recommendation was to initiate a screening process that concentrates the sampling on those individuals most likely to be at risk. That is, women from 15 to 45 who are heavy subsistence consumers. We will get a distribution of mercury levels in their hair, by section, which will provide a measure of the variance throughout the year. If this distribution has a fairly significant segment of the population in the 8 to 10 ppm, rather than the 90% that have less than 1 ppm that we saw from this initial 200 sample, then that will give some cause for concern. That is, is there an at risk population that may cross this 10 ppm theoretical threshold? If that's the case, and there is a significant increase in the methylmercury in the biota that they consume. Then those two facts together would give us some concern to kick in a monitoring program. But, Dr. Kosatsky's own work in Canada clearly indicates just knowing the mercury level in the biota doesn't tell us what's going to be taken up in the hair sample. It is difficult to establish those two links. Essentially we're not setting up a monitoring program, we're doing a "risk assessment." The first phase fairly clearly indicated that the population was not at risk. There were 3 or 4 individuals of 200 with levels between 4 and 8 ppm, which is not even close, in my opinion, to the 10 ppm level that would cause concern.

Concerning the signal versus noise aspects, (or if you come from the social sciences, disturbance terms, and the structural equation model) we are not going to try and address that problem because we don't have the controls to find the independent and dependent relationship between the variables we're concerned about. As has been mentioned, all these disturbance terms that affect that relationship are: 1) extremely hard to measure, and 2) expensive to quantify in any reasonable amount of time. Is that a correct summary, Tom?

Tom Kosatsky: Yes.

Anton Scheuhammer: But I think that the real question is here, is why is this even part of this project? Because there's no way we could ever link dredging with the outcome. So MMS probably should be doing this anyway, because there's a lot of other things that affect methylmercury in the environment, not just this. This would be only a minor one.

Ray Emerson: I agree across the board, that it's going to be very difficult, if not impossible, to establish a link that is going to be critical to the community of Nome regarding the dredging activity. But the important thing is that we have the proper database from which individuals can make decisions. So if the target group at risk knows where they stand with respect to the mercury issue, they know what to expect from the foods they are eating, with the respective resources. There is a multitude of food out there, and there are different parts to eat. They could make very intelligent choices that would be for the betterment of the entire community.

Donald Callaway: That's what we're going to do, Ray.

Ray Emerson: This was done in Canada. I think that is what we are trying to achieve through education, so those people will make the right choices, even though the mercury level in the environment increased.

Donald Callaway: That is what we are going to do. The second sample is going to take the distribution of the parts per million over time. Then whatever that distribution is will be relayed back to the individual. You asked am I satisfied with the data we have now, and my answer was, maybe. My best guess is the second round is going to show that the distribution doesn't lean towards this 10 ppm threshold. There may be some variance within the year, but that the overall mean will be pretty close to what was shown from the 200.

Ray Emerson: I got the impression from Tom, because we can't make the connection with dredging, and I'd agree, that there's no reason to continue to pursue the question. Is that what you're saying?

Tom Kosatsky: No, my point was that for purposes of public health, or for purposes of public reassurance, that it may be useful to monitor metals in people. But for making a link between the dredging and what's getting into people, that is of no benefit.

Ray Emerson: Well, let me put it this way then. There is a possibility of a contribution being made, would you grant that?

Tom Kosatsky: That dredging could increase levels?

Ray Emerson: Yes, that it could contribute to mercury levels, in people in the population?
Tom Kosatsky: Well, anything is possible. I think that the likelihood of that is extremely slim. In order to monitor that possibility, we need to begin and continue to take measurements of mercury in foods related to the areas to be dredged. And foods which are eaten by people.

Ray Emerson: Let me put it this way then. If there isn't a concern with the present activity that's proposed, in other words, it if isn't linked, I don't know who's coming forth with the funding to look at a question that may still be of valuable importance. Because if there isn't a dredging link, I can assure you, then it's not going to be funded by MMS.

Donald Callaway: Well, we could start measuring iridium, you know.

Dick Prentki: Well, let's throw a caution in at this point, it was sort of implied in this discussion that the proposal is for one dredge. If we have a lease sale, we are assuming one or two dredges. We are not proposing to limit the number of dredges which may be out there at one time. If we're going to control any environmental effects, which can be a stipulation for monitoring and baseline, it's not going to be by limiting the operation to only two dredges out there.

Terry Haynes: Ray is trying to get somebody to respond on their satisfaction with the sample of 200. I don't know enough about that project yet to draw a conclusion. But I do understand that there was a 50% refusal rate? I'm wondering – we do have statisticians here – if in other kinds of scientific endeavors, if there was a 50% rejection rate of your samples, would you easily be satisfied with the results of your study? I guess that's a question perhaps other people could respond to.

Donald Callaway: Well, we try to address that in the community meetings. That is part of the reason I didn't give Ray a straight yes or no. Fifty percent is way too high. On the other hand, the question is, has that 50% refusal rate interjected systematic bias? The *post facto* discussions we had with the people that did the samplings seemed to indicate that it hadn't. That is, that it hadn't biased it towards an age group, and clearly hadn't biased it by ethnicity, since those parameters were quite close to what we know from demographic surveys.

In their opinion, it didn't appear to bias it towards the non-subsistence users. So that on one hand you're right to be concerned that there was a 50% refusal rate, although the refusals were idiosyncratic for a number of different reasons. I think the second follow-up study will put to rest that concern fairly carefully. In general, I feel fairly confident from these first 200 samples that we have low levels in the population. I don't perceive any systematic bias in the sampling.

Terry Haynes: I think that is a good point. If we do follow it up on a carefully selected segment of that population, we all agree, especially the people from Nome, that we want to minimize the burden that is put on that population, by asking people to participate in more studies. But it seems to me that a follow-up project that looks specifically at the high risk population might be warranted, and certainly would not pose major impacts on the population. And might put the issue at rest.

Donald Callaway: It is my understanding that that's already been funded. It was incorporated in the initial funding. Am I correct on that? So that will be completed regardless. So that funding is not an issue in that case. We plan to go ahead with the second phase.

Bryan MacLean: Well, I was ready to go home here about 15 minutes ago, and all of a sudden I woke up. Tom's remarks kind of caught me a little bit off guard, we didn't hear a whole lot of that discussion when we were having the work session. It seemed to me when we left the work session that we were not really sure what the confidence level is in the current set of data. So we were going to go back and verify that. We're going to look at the target population that is the most likely to be affected. With the sample that we now have, we don't know what that target population is that we actually sampled.

Are they really heavy subsistence users, or are they occasional subsistence users? Is it by choice, or is it they have no other alternatives? So the second round of testing will focus in on that group which is likely to be the highest at risk. That will tell us where we stand in terms of what risk window are we looking at.

If we have that high risk there, then we need to monitor the resources that are being used to determine: 1) whether or not there has been an increase in those, and 2) whether we need to be concerned with human consumption. If there is, then we are going to go back and look at those people again. Where I start to get a little bit concerned is that we are not sophisticated enough to develop a sampling or monitoring program. We kind of want to sweep it under the rug and say, well, we can't monitor it so we don't need to look at it, so it's not a problem.

We heard from one of the statisticians a little bit earlier that we can't really separate the noise out from the greenhouse effect, but we know it's there. So I get uncomfortable when we really start to say it's not something we want to monitor. I think we need to look at it and we can't ignore it.

It's also a public perception problem. Whether or not there's any scientific basis for it, yes, there is a problem, or no there's not a problem. In the minds of a lot of folks in the area, there is a problem. In the minds of others, there isn't a problem. For those who feel there is a problem, no amount of discussion here by us esteemed experts is going to convince them otherwise. So it's partly a P.R. thing. We need to go back in and say, we're not sure, let's check, yes there is a problem, no there's not a problem, if this happens we need to take some additional steps.

It has to come from the community, and you're not going to convince the people up there to support the program unless they are satisfied, at the community level, that there's not a problem. You are not going to do that by saying we are not going to monitor.

Jerry Imm: Could I add something, please? Irene, and Matthew, and some of us talked out in the hallway a while ago about this very problem. In fact there's a high risk community there, the lower end of the socioeconomic scale that Dr. Marsh was talking about, King Islanders, essentially. What we thought we would do is actually go in and take a sample of hair from some of those people, women of child bearing age, and have those analyzed using the segmental analysis technique. Also go back to some of the women that were in the original sample, the ones with the higher levels, and actually take some additional hair from them, and do a segmental analysis to see if we again have a problem with that spike, or whatever that we're looking for on a seasonal basis. But, nonetheless, we were already thinking about doing exactly what you suggested, looking at that group that may be at higher risk from eating more subsistence food, and some of the people from Nome sitting in the audience do at this present time.

So I think we're going to look at that. Then after we get those data, it's probably then wise to decide whether we need to go further or continue as Don Callaway suggested, or what. But I think right now that we are looking at collecting additional information soon to address that problem.

Donald Callaway: I don't think what is being said is contradictory. That is, just because we can't separate the signal out from the noise doesn't mean that in terms of public health, if we see a distribution, whether it's King Islanders or within the Nome community, of people at that 10 ppm threshold, and that we can't determine what the effect of the dredging is, it still can be a matter of public health policy to advise those people. If that distribution does occur, and if there is an increase in the level of the subsistence species, it might be good, if they are pregnant or planning on being pregnant, to limit their use of subsistence foods. But until that distribution is revealed, and it certainly wasn't in the first 200 study, until that point it's counterproductive to make those kinds of suggestions.

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What I'm saying is that it is not necessarily true that we have to be able to separate "the signal from the noise."

Chuck Mitchell: That's what Tom said earlier.

Bryan MacLean: I'd like to address Ray's comment a little bit earlier. We said unless we can draw a link from the dredging activities to this issue, it'll be tough to get MMS to address this issue. In the minds of a lot of people in the Nome area, there is a direct link. It would be to industry's benefit to either show that yes that link is there, or no that link is not there. Once that problem is put to bed, then we can all start to rest.

But it's not just specific to the Nome area. There are village residents in the surrounding areas that are getting concerned about this. They are starting to ask the same questions: What's going on up there in Nome? Is there a mercury problem, or is there not? If that comes down in our area here, are we going to be exposed to be same dangers that they are exposed to up there? So you need to look a little further down the road, and say we have a P.R. problem here. We may have a health problem, let's take care of both of them at the same time.

Robert Fagerstrom: I would like to just say one thing. We appreciate Bryan's comments. I'm the president of the corporation, and we have 50% of our shareholders living in Nome. I think that we can communicate very well with them. I think the majority of us feel that we would know if there are problems up there. I was born and raised up there, and Matt was there, and we would hear of things. I think that things are getting blown out of proportion. We think we've got some good numbers. We just have to take a second look, and we should get back onto what we're doing here today.

Donald Hood: Thank you very much. I think that is a favorable closing comment. I think we've beat this issue about as hard as it needs to be kicked today. Unless there are some points to be made outside the human health range that we have skipped over because of use of time, perhaps we should bring this to a close. Are there any other comments?

Tim Holder: This is a question that jumps across to all the issues here. I've been wondering for years, if there's ever a point at which people could conceive where you wouldn't have to monitor. Perhaps the individuals who headed each session could maybe give a short comment on that.

Ronald Eisler: One monitors while there is an ongoing dredging operation. I would suspect one always monitors while the operation is in progress. The purpose of the monitoring is to establish when the ecosystem has stabilized. How long it takes, I don't know. We know that it's going on for at least two years. In some cases of mercury contamination, mercury is detectable a hundred years post-spill. It is one of those very open-ended questions.

Donald Hood: John, you were about to respond.

John Trefry: I think Eric and I would both feel that if you had a sufficient number of years of data that showed no changes, and a constant trend, that you could stop monitoring. How long that would be, I don't know, 3 to 4 years, 3 to 5 years?

Donald Hood: Bob Smith or Sathy Naidu?

Sathy Naidu: I want to make sure that you understand the difference between contamination and pollution. These two words have been interchanged with the same connotations in many discussions. An environment is contaminated when the baseline values, the natural values are no longer there. There's no environment on earth which has not been contaminated. An environment gets polluted when you see an adverse impact on the biological population. If it is manifested in any way, you see an adverse effect, and the environment is polluted. So if the environment is not polluted, then that's the time when monitoring should stop.

Donald Hood: Thank you.

Tim Holder: I'm wondering about the fact that the dredge is always moving around, and is may be getting into different sediments. Isn't there always the possibility of getting into different materials in those sediments that are possibly pollutants? So, in that sense, do you always have to keep monitoring to check what's coming out? Or do you get enough points on the graph, whether it's five years, or ten locations, or whatever? Are you saying that within an area that's the size of this sale area, and let's say all the area was leased, do you get enough points on the graph where you can say, well, we don't think they're going to get into different sediments from a geologic standpoint?

Sathy Naidu: I think I must take back my word. I shouldn't say that monitoring should be stopped. You know, it should be continued over the operational period, the twelve years or so. But how often, how frequently, that should be determined by how much the environment is getting polluted.

Chuck Mitchell: It seems, over the years, that the frequency or the sensitivity of the monitoring changes, the scope of the monitoring changes. But it usually does continue. For instance, if you're going to put in a fixed discharge, generally you set down a program, you establish baseline conditions, then the facility begins to operate. You maintain the baseline station locations and sampling procedures, until such time, say 2 to 4 years, that you can either see there is no effect, or there is an effect. At that point in time, usually the operation of the facility is modified to bring it back to a "no effect" kind of situation. Then you go to something like an NPDES monitoring, where for instance you then measure the discharge at the dredge in this case. That becomes your trigger, if the water quality changes significantly from what it has in the past, that you've already as documented as having "no effect", then it becomes your red light and your gong.

Donald Hansen: I think that the sediment group mentioned 3 or 4 years. That sale area is divided into an east and west area. If they were dredging where they are now, and in that area, for 3 or 4 years, and they didn't find anything, but they moved over to the more eastern area, would that justify continued monitoring? If you didn't see any effect in the first segment of the area, but you went over to the other. It's a different location, in other words, if there were different sediments or whatever.

John Trefry: I think compromise is always the best way to go. Maybe the comments that Chuck just made a minute ago, that you scale it down, so you'd have some continuous monitoring. Certainly if you shifted areas, you might intensify or so forth. I think the compromise situation sounds good, some minor continued monitoring at the dredge site, maybe not regionally so much any more.

Donald Hood: Well, now we've about run out of time. So I think it's time to close this discussion. I have the pleasure of thanking all you people for your attention. I think it's rather remarkable that so many people have retained interest throughout the meeting.

It's been a good scientific discussion. We've had very little outside disturbance or pressures. And I think maybe that's why we're all still here, laboring with the problem. It's a very difficult problem to analyze and do something with. We have given a great amount of information and it will, I feel, be useful in developing a baseline and monitoring program for this proposed mining operation off Nome, Alaska.

There will be reports resulting from this workshop, as you know. I guess the dates were indicated earlier. There will be a draft by February and final by July. We will be shooting at these two targets. There will be communications with the chairman. If you want information out of this meeting, I'm afraid you'll have to depend on your own resources for information communicated at this meeting, until the report is out.

I has been a pleasure for me to chair this workshop. I am indeed honored to be asked. It has been rewarding for me to review past work done on the Bering Sea and to work with you in an attempt to devise a baseline/monitoring scheme on the potential impact of subsurface mining operations in this very interesting environment.

It would be appropriate at this time to summarize the findings of this workshop, however, because of the restraints of time and the difficulty of drawing together a meaningful summary with so many ideas continuously being generated, this will not be done. The summary will be prepared and included in the written document.

I wish to thank the MMS for sponsoring this meeting, for the MMS staff for their contribution to our deliberations, to chair- and co-chairpersons of the sessions who did a good job of pulling information for their specific sessions together, to all attendees to listened and contributed to the discussions, and last, to Kathy and Chuck Mitchell for organizing and managing the conduct of this efficiently run workshop. Thanks to all and may our deliberations have value in the long-term protection of our marine resources, specifically the Eastern Bering Sea, near Nome, Alaska.

CHAPTER 7

SUGGESTED BASELINE AND MONITORING PROGRAM FOR NORTON SOUND GOLD DREDGING OPERATION ON FEDERAL LANDS NEAR NOME, ALASKA

Donald W. Hood, Ph.D., Workshop Chairman

SUGGESTED BASELINE AND MONITORING PROGRAM FOR NORTON SOUND GOLD DREDGING OPERATION ON FEDERAL LANDS NEAR NOME, ALASKA

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The presentations and emanating discussions occurring in the five sessions of the workshop accompanied by the results of the "Review and Recommendations Working Session" represent a substantial body of information on baseline needs and monitoring requirements for the planned gold dredging operation near Nome, Alaska. While this material, presented earlier in this report is thorough and far reaching, a cohesive plan for baseline or monitoring studies may be difficult to extract without further consolidation. It is the purpose of this section to draw together a comprehensive baseline study that best fits the recommendations of the workshop and yet provides a reasonable sampling regime on a time scale compatible with circumstances in the Nome area.

During the workshop there seemed to be continuing difficulty in separating studies needed to establish a baseline on which monitoring would be designed to indicate changes and the monitoring itself. Since these are distinctly different requirements, it is my intention here to first establish a meaningful baseline study, then to follow this with a program compatible with the workshop discussions for monitoring. To accomplish this it is first necessary to establish what we mean by a baseline study.

The most succinct expression given at the workshop was that of Dr. Woollcott Smith, which states, "For me a baseline part of the survey is doing whatever you need to do to get ready to monitor the effect. If it means measuring ambient conditions, that's part of the baseline. If it means tightening up your quality control, that's part of the baseline. If it means devising new measurement techniques, that should also be part of the baseline. The criterion for judging a baseline study is - at the end of the baseline period do you have the data and measurement resources available to detect the impact in the monitoring stage of the study?"

In reviewing the workshop deliberations it becomes apparent that the session on human health stands apart from the other three sessions in most aspects of baseline studies. It was generally concluded that, while humans are one of critical elements of concern in environmental degradation, they are poor subjects for indicating the state of the environment because of their capacity to change food habits and difficulty of obtaining representative samples of the population. Human health considerations represent problems unique unto themselves and must be considered independently of the environmental issues. The recommendations of the human health session stand alone. No attempt will be made here to coordinate these proposed studies with those of the other sessions.

Water column and sediment chemistry, food chain and bioaccumulation, habitat alteration, and statistical and sampling techniques sessions have made many independent recommendations. In some cases the recommendations are redundant or overlapping, do not contain a logistics plan and are incomplete owing to time restrictions or available information to the working group. It is my purpose here to consolidate these recommendations into a practical working program that will allow the important recommendations of each group to be carried out within a single plan, including logistics. The importance of an interdisciplinary approach was emphasized throughout the workshop.

The value of interdisciplinary research being carried out on samples obtained at the same stations, preferably on the same platform, has been shown in many situations, but, perhaps, none better than on the PROBES program (Continental Shelf Research 1986) conducted on the Eastern Bering Sea Shelf between 1978 and 1982. Baseline studies that consider all the components of the environment, to the extent possible, are much more valuable in considering natural fluctuations, and thereby allowing identification of effects because of man's intervention, than are discrete disciplinary studies. It is my personal opinion that the workshop inadvertently overlooked the powerful support of physical oceanographic data in their recommendations for a baseline study program. This omission can be easily corrected by including these observations in Program A and will require little increase in time if a 24 hr use of the oceanographic vessel is made when in the lease area. A station grid for physical oceanography is indicated in Program A below.

The baseline study presented here attempts to bring together the recommendations of the workshop, excepting the human health session, into an integrated interdisciplinary program that will be carried out by an interdisciplinary team of marine scientists.

SUMMARY OF WORKSHOP RECOMMENDATIONS FOR BASELINE STUDY

TIMING

Two types of programs are proposed: Program A. A broad based program to include extensive detailed sampling at specific stations to establish normal range of conditions and annual variability. These studies should extend over a period of at least 3 years before dredging commences. Program B. This program is designed to establish seasonal trends and is particularly needed to determine the population dynamics of the food chain organisms. These studies should be carried out quarterly. The winter quarter could probably be eliminated if the present practice of only summer time dredging is continued.

LOGISTICS

Program A should be conducted on an oceanographic vessel equipped with state of the art navigation equipment capable of establishing accurate station locations; laboratory space for conducting simultaneous interdisciplinary studies; salinity-temperature-density profiler as onboard equipment; box or vibracore capabilities; a Doppler *in situ* current measuring device; and, capacity to employ side-scan sonar survey of the lease area.

Program B may be conducted aboard a good seaworthy local craft that has, or can be equipped, with a portable winch and navigational capabilities to locate stations within 100m of those in Program A.

SAMPLING PROGRAM

Side-Scan Sonar

During the course of the three year baseline study to be conducted prior to the dredging operation a side scan sonar survey should be made of the entire lease area.

Rationale: One of the major changes that we know will happen during the dredging operation is a change in bottom topography. The natural condition should be ascertained in order to determine the extent of change and its duration.

Physical Oceanography

A grid of stations about 10 nautical miles apart should be established between 164° and 166° West longitudes and 64° N latitude to the coast that will be occupied during Program A to help establish the physical regime for this area. Three regional stations (referred to later under water quality and sediment sampling program) should be established at 164° W longitude and 64° 15' N latitude; 165° W longitude and 64° N latitude; and, 166° longitude and 64° 30' N latitude. These will represent control stations for the lease area and are selected to fall out of the area of probable effects owing to the actual dredging operation. The first two stations are located at positions not sampled in previous surveys of the area, but the third station is near Station 26 of the Hood *et al.* survey (1974) where the sediments consist of relict gravel and local patches of recently deposited sand and mud (Figure I-3).

Data to be obtained at these stations should include salinity, temperature, and density profiles from the surface to the bottom; Doppler current reading on station and while underway; and, dissolved oxygen and light transmittance profiles.

Rationale: In order to detect any effects on the environment that may result from the dredging operation it will be necessary to establish the natural variations in this environment. Interannual changes in the physical regime in this are of the ocean can be profound (See Continental Shelf Research 1986) owing to changes in weather patterns, storms, river runoff and local ice conditions. This program will help determine these changes.

Water Quality and Sediment Chemistry

A minimum of 17 stations are proposed by the water quality and sediment chemistry session. Of these, three are regional control stations as discussed above, two are water quality stations and the remainder (12) are for sediment sampling. The location of these stations is indicated in Figure 2-1. The station locations, while generally falling in the same pattern as those occupied by Naidu *et al.* (1989) in the lease area, the locations are not duplicated. Also the Station locations occupied by Hood *et al.* (1974) were not available at the workshop. It would appear desirable for the research team undertaking the baseline survey to establish the suggested number of stations at sites previously occupied to the extent possible in order to take full advantage of the historical data available. Of particular interest would be the lines of Stations 1-5 and 6-10 near Nome that were occupied by Hood *et al.* (1974) shown in Figure 1-3. Unfortunately much of the Hood *et al.* survey was more west of Nome than the present lease sale, but the overlap appears to be sufficient to give valuable historical information on environmental changes that may have occurred over the past 15 years in this area. Detailed sampling and analysis schedule for water column and sediment chemistry is presented in Chapter Two of this report.

To comply with the strong recommendation of the Food Chain and Biological Accumulation Session it is recommended that at least two of the stations selected for interstitial water analysis be converted into deeper profile stations. This will require the use of a vibracoring device in order to obtain samples to the depth of planned dredging of 3.5m.

It appears that the water column and sediment chemistry baseline survey program can be mostly carried out in the Program A sampling plan, but seasonal changes in the water column chemistry for cadmium, copper, lead, and mercury should be followed and can be sampled on Program B along with the Food Chain and Bioaccumulation studies.

Rationale: This program will provide baseline information on the levels of minor components of concern that are present in the water column and sediments of the lease area and at control sites. These parameters can be monitored during the dredging operation to determine if any significant alterations of the environment have occurred.

Food Chain and Bioaccumulation

The recommendations of this group was primarily concerned with monitoring the actual dredging operation. For this monitoring to be meaningful, a baseline level of the components of concern in the indigenous animals to be monitored will be necessary. To establish the baseline concentrations of the minor chemical components of interest in these organisms it would appear that the stations occupied by the physical and chemical program should also be occupied by this group on the annual large ship program. In addition to the sampling and analysis proposed in the recommendations of this group for the benthos and fishes, it would appear valuable for this group along with the habitat alteration group to repeat the sampling and identification of the biomass as was done in this area by previous studies to help establish any changes in species composition or diversity that might have naturally occurred during this period.

Sampling to establish any seasonal differences in the chemical composition of the selected species will need to be done from local small boats under Program B. The same stations as occupied under Program A should be used here. Sampling for birds and mammals will have to be arranged separately from the seagoing program.

Rationale: The baseline program proposed will establish the limits of variation of the concentrations of cadmium, copper, lead, and mercury of selected organisms under natural conditions. Changes which may occur during the dredging operation can then be measured against this background to determine if significant detectable changes have occurred.

Habitat Alteration

The recommended study elements of the habitat alteration session fit entirely into Program A, except for the deployment of sediment traps. These traps (number of arrays are not stated, but I would assume the three regional and about three others to be selected depending on the side scan radar results would be adequate) could be deployed on Program A and retrieved later in the year on Program B.

This group emphasized the need to know if the biota composition might change as a result of the dredging operation, thus indicating the need to establish what changes have occurred naturally with time in this area. This places further need to establish stations for acquisition of biomass data that coincide with the historical stations recommended above.

Turbidity of the water column is of particular interest to this group. The transmittance measurements recommended under the physical oceanography studies may be adequate to establish the background levels to be expected for the area, however it would be good for at least one season to follow the Program B sampling program to see if natural differences occur seasonally. Since one of the measurable effects of dredging may be a reduction in primary productivity in the dredging area because of light limitation to the phytoplankton, emphasizes the need for these measurements.

Rationale: Changes in the bottom topography, changes in surficial sediments and the creation of a turbidity plume are clearly going to occur as a result of the dredging operation. It is necessary to establish where materials are going and the nature of the habitats that will be affected prior to operations. The natural conditions found can then be monitored for any changes imposed by the dredging operation.

SUMMARY OF RECOMMENDATIONS FOR MONITORING

Much of the data collection during the monitoring process will be an extension of the baseline studies. Included in this will be the physical oceanography, the water column and sediment chemistry, and most of habitat alteration baseline program. The food chain and bioaccumulation studies undertaken during the base line will need to be continued during monitoring, but will need to be expanded to include deliberate exposure of selected organisms within the area of influence imposed by the operating dredge. This will be done by placing trays containing appropriate organisms (probably clams such as *Spisula*) in positions where exposure to the plume of the dredged material will occur. Associated with the exposure studies data will need to be collected on transmissivity and particulate content of the water column.

Logistics for the monitoring program will be about the same as for the baseline study for the first two or three years of operation. Depending on the observed results the program can be modified in subsequent years to accommodate any change or lack of change observed.

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APPENDICES

APPENDIX A

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APPENDIX B

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